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Thomas

" EVOLUTION OF SOUND" EVOLVED

A REVIEW OF THE ARTICLE ENTITLED

"THE NATURE OF SOUND"

IN

"THE PROBLEM OF HUMAN LIFE"

BY

M. J. THOMPSON, A. M.

Professor of Sciences in Garfield University, Wichita, Kansas

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PREFATORY NOTE.

Five of the following chapters appeared in *The Disciple*, a monthly magazine published in Cincinnati. Of these the first three were signed "Clarence."

Dr. Hall made a long reply to the first of these articles, merely acknowledged in his paper the receipt of the second, and said nothing at all about the others.

I can not but express my surprise at this silence. The Doctor has always shown himself anxiously ready for a discussion. For years he has been making the most audaciously personal appeals to the leading physicists of the world to pay some attention to him. But as his appeals have all been in vain, I thought perhaps he might be willing to amuse himself for a while by pursuing smaller game. But seemingly I was mistaken. Humiliating as it would naturally be to my feelings, I would not demur to accepting the explanation that the Doctor considers me unworthy of notice, if he had not said this about me in his reply to my first article:

> "Our sympathy with him is all the more intense, in that he evinces in his writings a degree of ability which might have proved a blessing to the world in advocating Substantialism, had his mind

chanced to see this philosophy in its true light, and had he not been misled by circumstances."—*Scientific Arena*, April, 1887, p. 175.

After all, I do not believe the Doctor is half so warlike as he thought he was.

JULY, 1887.

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INTRODUCTION.

In the summer of 1881, being then quite a young man, I saw in a newspaper a review of "The Problem of Human Life." It was quite eulogistic, and declared that the wave theory of sound had been completely overthrown. If this were true, I thought it an interesting fact, and immediately secured a copy of the book. I had learned from the review something of the drift of its teaching on the nature of sound, and was predisposed to accept its revolutionary views.

I expected sometime to devote myself to scientific studies. If all the leading theories of science were to be revised, it occurred to me that I had been born into the world just in time. If all scientific men were now "at sea," a young man might launch his little bark with more hope of success.

I read the volume through in a few days. I was confused, but not convinced. The book was of undoubted strength and plausibility. But a suspicion would linger in my mind that a fallacy was hidden somewhere in the argument, and which I did not see. I had seen many mistakes on minor points, but the book seemed strongest on essential features. I saw that sometimes he had entirely misapprehended the teaching of the wave-theory. Its argument, I felt, was mainly destructive. Its constructive argument was far from satisfactory. I disliked exceedingly the spirit in which the work was written. I re-read the discussion of the principal points. The mists cleared away. It was somewhat humiliating to find I could be befogged so easily. I consoled myself, however, by thinking that many older heads than mine had been duped by the same thing. The book was plausibly written, but it did not seem difficult to answer. I have since seen that a large share of its plausibility comes from its extraordinary volubility and assurance.

Being satisfied with my examination, and retaining my allegiance to the acoustical "faith of my fathers"—a creed that I had half-expected, even hoped, to abandon—I paid no more attention to the book for years.

Something over a year ago a friend urged upon me the advisability of putting these thoughts into print. Though having no special longing for authorship, the suggestion struck my mind favorably. I know that I am dealing with an ephemeral question, that the next generation will know nothing of "The Problem of Human Life," or of this review, either.

It is amusing to read the statements of the "substantialists" (as they call themselves) about "the magnificent fight that is being waged in the scientific world." All the fight that I know of that is being waged is in one newspaper office, in one city, by one little coterie of men. The remainder of the "scientific world," which, however, is several thousand times the larger, moves placidly on, and literally knows nothing of the "fight," but is studying into the mysteries of nature, discovering facts, making inventions, generalizing its knowledge.

To avoid allowing this work to become too voluminous, I have purposely dwelt at length upon only essential points.* The book, of which this little volume is a review, is written with remarkable volubility and in utter disregard of all logical arrangement. Every argument is stated with great fullness, and occurs many times over in all its fullness. Seemingly, any argument can be made to fit anywhere, and accordingly any argument is lugged in at most unexpected times and places.

Dr. Hall is author, not only of "The Problem of Human Life," but also was editor of the *Microcosm*, and now edits the *Scientific Arena*, which succeeded the first journal. In this review I have referred to them as freely as to the volume mentioned, for they are but little more than the expansion of that which was first published in his book. When page alone is given, reference is meant to "The Problem of Human Life;" other references are properly indicated.

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* Blunders like the following, not being in the direct line of this review, need no discussion. On page 197 he speaks of "a lump of ammonia." But ammonia is a gas, and we can not have a "lump" of it. On page 251 he speaks of "the air in hydrogen gas," and founds an argument on this supposition. But pure hydrogen contains no air, and the atmosphere contains no free hydrogen. Such phrases as these are allowed to stand in the revised edition of 1878, which he says was "wholly rewritten," and hundreds of "substantialists," who think their intellects are so exceedingly keen that they can find a blunder in almost every line of a book written by a "wave-theorist," have gone over these phrases many times and seen nothing wrong.

In the preface to his book, Dr. Hall says:

> "Prior, however, to undertaking the task of breaking through the entrenched works of the evolutionist, and in order to prepare the reader for placing the proper estimate upon these so-called scientific theories which assume to overthrow religion—such, for example, as Mr. Darwin's doctrine of man's development from the monkey—I resolved, as an example of what might be expected in the future, to attempt the overthrow of one of the universally accepted theories of science—a theory which has never been called in question by any writer on the subject, and which is considered to-day, by all scientists, as as firmly established as the Copernican Theory of Astronomy,* or as little doubted as the law of gravitation, namely, the Wave-Theory of Sound, out of which has been developed the undulatory Theory of Light, and the more recently constructed theory of Heat as a Mode of Motion."—P. 5.

That is to say, he had a case to make out, and there were to be no scruples as to the means employed. Having found a theory that he believes is more generally conceded than any other, he prepares to make out a case against it. It makes no difference whether the theory be true or not—that is not involved in his purpose.

The mind instinctively prepares itself to listen to reasoning that resembles the argument of a shrewd but unscrupulous lawyer before a jury. Even the sharpest lawyer, in his eagerness, will sometimes overreach himself, and leave an impression on the minds of his hearers the opposite of what he intended.

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* Dr. Hall is not fortunate in his illustrations. Quite a large coterie of men do object to the Copernican Theory of Astronomy; and since writing this preface, Dr. Hall himself has made an attack on Newton's Law of Gravitation.

Dr. Hall has done this. After reading this statement in the Preface, one turns to the opening chapters, expecting to hear some "special pleading." Quibbles and sharp practice will be the order of the day. At least that was the impression left on my mind.

One finds what he expected to find. A mind naturally very acute, has bent every energy of its being to gaining a verdict against the wave-theory of sound.

He knows the jury. They are intelligent men, but not specialists in acoustics. They have never read an exposition of the wave-theory (unless it be a few pages devoted to it in some school-book), and probably never will. They are believers, and they gain the impression that the undulatory theory of sound is the foundation of most of the skepticism of the day. The probable verdict can be foretold. And when the verdict is pronounced against the wave-theory, Dr. Hall and the jury are astonished to find that the scientific world does not care.

If a farmer should publish an article on the best method of raising a certain kind of grain, and all the city people in the world, who had never done any farming, should declare the method to be useless, and worse than useless, what would that farmer care? If other farmers endorsed it, he would care nothing for the lurid invective that went up from the cities.

After all, it does make a difference whether or not a man knows anything in regard to a matter about which he is talking. Such a common sense way of looking at the converts as this, however, never occurred to a "substantialist."

In a foot-note, Dr. Hall gives this explanation of his attack on the law of gravitation:

> "An influential critic—a professor in a leading college—about a year ago ridiculed our attempt to break down the wave-theory of sound, and said, 'He might just as well attempt to break down Newton's theory of gravitation, both alike being established by mathematical demonstration; and we should not be surprised if this iconoclast would attempt Newton's law for his next crusade.' How prophetic! Acting upon this cue, we examined Newton's law, and compared its mathematical demonstrations with those of Tyndall and Helmholtz for the wave-theory, with the result now being made public through these discussions."—*Microcosm*, Vol. II., p. 128.

That explanation does not leave the impression on the mind that the investigation was carried on with intellectual honesty.

Physicists have considered the possibility of the overthrow of the accepted law of gravitation, and have felt no repugnance to the thought. Prof. Tyndall says:

> "While I have endeavored to place before you, with the utmost possible clearness, the basis of the undulatory theory of light, do I therefore wish to close your eyes against any evidence that may arise of

its incorrectness? Far from it. You may say, and justly say, that a hundred years ago another theory was held by the most eminent men, and that, as the theory then held had to yield, the undulatory theory may have to yield also. This is perfectly logical. Just in the same way, a person in the time of Newton, or even in our own time, might reason thus: 'The great Ptolemy, and numbers of great men after him, believed that the earth was the center of the solar system. Ptolemy's theory had to give way, and the theory of gravitation may, in its turn, have to give way also.' This is just as logical as the former argument. The strength of the theory of gravitation rests on its competence to account for all the phenomena of the solar system. On a precisely similar basis rests the undulatory theory of light; only that the phenomena which it explains are far more varied and complex than the phenomena of gravitation."—*Notes on Light and Electricity*, p. 126.

When a man can bring forth arguments strong enough to overthrow Newton's theory of gravitation, they will receive examination without their author being compelled to attack anybody's character to attract attention, and scientific men can not be compelled to pay attention to rubbish by making personal attacks upon them.

Dr. Hall's fury against "current science" is very great. But this is nothing new. Such tantrums are common. Every few years some one comes forth riding a hobby in science. He wants all scientific men to ride the same hobby, and, like all hobby-riders, he wants all others to ride behind. After examination, the great majority of scientific men decline to ride at all. Then the sky becomes lurid with the fitful glare of the scathing denunciations of "current science."

But the scientific world moves steadily on, while the hobby and its rider pass to the Lethean shore, forgetting the world, and, sad to say, forgotten by the world.*

A few years ago Dr. J. B. Dods discovered what he called "Electrical Psychology." † The drift of his teaching may be gained from a few extracts from his book:

"From the extracts which I have read from the Auburn papers, you will at once perceive the power and glory that hover around this science, and the importance which is claimed in its behalf as one of the greatest blessings ever vouchsafed to the human race."—p. 26,

"To establish the fact that electricity is, indeed, the connecting link between the mind and the body, I would in the first place distinctly remark, that mind can not come in direct contact with matter. My mind can no more directly touch my hand than it can the mountain rock."—p. 55.

"Though there is but one grand cause of disease, which is the electricity of the system thrown out of balance, yet there are, nevertheless, two modes by which this may be done. It may be done by mental impressions. And it may also be done by physical impressions from external nature."—p. 71.

"Independent thought and fearless expression have ever drawn forth the scoffs and sneers of that portion of our race who have adopted, without investigation, the scientific opinion of others. I refer to those only who have received their ideas by inheritance, as they did their real estate. For the one they never labored, and for the other they never thought."—p. 34.

---

* Dr. Hall stultifies himself, however, and shows that he does not believe his own accusation that the scientific books are but bundles of errors, when he makes such statements as the following: "Dr. Lord would do well to commit some elementary work on natural philosophy or astronomy before discussing the distance of the fixed stars as judged by the velocity of their light or their appreciable parallax."—*Microcosm*, vol. i., p. 317.

† Dr. Dods' book with this title was published in 1852, by Fowler & Wells, Boston.

"Had not some master spirits dared to freely speak and write their thoughts, then those pretended friends of science, who now oppose everything that may appear to them both new and strange, would have been destitute of that knowledge they obtained from books; and not daring to think for themselves, they would have remained in mental night. It is by daring to step aside from the beaten track of books, and bringing forth from the dark arcana of nature into the light of day some new truth, that we unite our mite to the common stock of knowledge already accumulated. He who denies us this grand right of our nature is a scientific bigot, and has yet to learn that even the school and college were only established to discipline the mind for action."—p. 36.

The writings of this man, like those of the "substantialists," have the true ring of intolerance. Because some expressed their doubts, he raised the cry of scientific bigotry. As though that other man had not as good a right to express his doubts as Dr. Dods had to publish his views. The right was no greater in one case than in the other.

Dr. Dods thought Electrical Psychology would revolutionize the world, that it would banish disease, pain and ignorance, and lift the world to an immensely higher plane than that on which it now lives. As we see, because the majority of scientific men looked upon his theory coldly, he was exceedingly bitter against them. He appealed, he said, from the scientific bigots to the general public, who were more open to new impressions. He gained recognition there. A committee of the United States Senate invited him to lecture in the Capitol at Washington. The names of the persons on the committee certainly command our respect. They are: G. W. Jones, John P. Hale, H. Clay, Thos. J. Rush, Sam. Houston, H. S. Foote, Daniel Webster.

If Electrical Psychology could have done one-half of what its discoverer said it could, it would have encircled Christendom by this time. It had public recognition, and if it had possessed inherent worth, the opposition of the whole scientific world could not have kept it down. The hobby has carried itself and its rider into oblivion, but "current science" remains.

Dr. Dods has this to say on the method of creation:

> "It is the most commonly received opinion in the Christian world that God made all things out of nothing. It is true the Inspired Book does not say or even hint this. It simply says, 'In the beginning God created the heavens and the earth,' but it does not add the words, 'out of nothing.' It is absolutely and philosophically impossible, in the very nature and constitution of things, that something can be made out of nothing. It implies, at the same time, a contradiction in terms. We can not form even a notion in our imaginations how much of nothing it would take to form the least imaginable something. I am speaking of 'nothing' in the strictest sense of the word. But using the word 'nothing' in its common acceptation, we can easily perceive how all things could have been made out of nothing. When all visible things are removed from a room, we say there is nothing in it—it is empty. Yet we know it is filled with air, because we continue to breathe. But if the air, by a force-pump, were removed from an air-tight room, we might, with much more propriety, say there is nothing in it; yet electricity would be there. If solid substances were, therefore, made out

of air, in an empty room, and nothing but electricity left, and if solid substances were produced from this ethereal and invisible fluid, we could, with much more apparent consistency, say that they were made out of nothing. In this sense, I grant that all things were made out of nothing. Paul says, 'The things that are seen were not made out of things that do appear.' Here he plainly states that the substances seen were made of invisible substances, or such as did not appear; for by 'things' he means only substances. If, however, it is said to create must mean to bring something out of nothing, I have only to say, that is not so; for it says, 'God created man out of the dust of the earth.' Here he created him out of something—it was dust, and yet it was creation. The Hebrew word rendered create, more strictly means to gather together by concretions, or to form by consolidation—but never can it mean to bring something into existence from absolutely and positively nothing. I therefore contend that all things were made out of electricity, which is not only an invisible and imponderable substance, but it is primeval and eternal matter."—pp. 121, 122.

Dr. Dods had a suspicion he was a martyr, and the "substantialists" are absolutely certain they are.

To see them posing as martyrs, and to hear their pathetic yet heroic words, is a touching scene. The only difficulty is that they can not secure any persecution: and who can be a successful martyr without a persecutor? Scientists have given them an open field. They can make as many converts as they please, and no scientist of reputation utters a word of protest or lays a straw in their way.

Martyrdom is a word having almost sacred associations connected with it, and it should not be debased by applying it to so free and festive an occasion.

A paper in Chicago, entitled "*Problems of Nature,*" an advocate of Spiritualism, had this to say* on the death of Prof. E. L. Youmans, editor of the *Popular Science Monthly:*

> "A few so-called scientific papers are published in this country, with a bit of circulation and a bit of pretension to fame. Only one of the number has a circulation of any consequence. All the rest are continued barely as the favorite of a college or wealthy person. All are an incumbrance to the owner, except the *Popular Science Monthly*, of the house of D. Appleton & Co.; and this is fast losing its circulation. Not less than half of it is gone already since our paper was started, and in another year its owners will call off the great copier of fashionable science. Its principal editor is dead, and his soul has caught an idea for the first time since its departure from the body. He was barely a spiteful defender of a bigot's opinion, and in no way did he contribute a speck to the world's knowledge. He was absolutely incompetent to see a fact of Nature, even when pointed to one; and a a beetle's tail possesses as much discernment of the operations of Nature. A childish and sychophant admiration for a wily bigot was his whole work, and in some cases he was actually worshiping a dunce or half-witted person as a great discoverer of the affairs of creation. He could swallow the whole Darwin Gospel at one gulp of his throat, and because a fancied bigot was swallowing it, too. He was as powerless to see a fact was not in it as a grasshopper, and he could only detest a person who could. With the exception of this pamphlet, given assistance by a great and wealthy book firm, there is not one scientific affair capable of obtaining its expenses from a public. In another year all are out of existence. There is not a writer on one of them who is fit to do more than copy and praise a bigot. Not one can see a fact or operation of Nature when informed what it is. All are as worthless as teachers of science as the dead stocks of grass."

---

*Feb. 1, 1887.

This editor can show letters from his admirers filled with as fulsome flattery as Dr. Hall can.

But another craze, much similar to Dr. Hall's philosophy, is now running its course among us, namely, the Zetetic Philosophy, or the theory that the earth is flat—advocated especially by John Hampden, of England, and William Carpenter, of the United States. These men believe, with Parson Jasper, that "the sun do move."

There is a remarkable parallelism between the circumstances of the two theories:

(1) *Both have a religious origin.* The Zetetic philosophers say there are certain passages in the Bible that assume that the earth is flat. Hence they oppose the Copernican system of astronomy as contradicting the Bible.

As between the Zetetic philosophers and Dr. Hall, the former have the best of the argument. If we say the Bible is always to be understood in the baldest literal sense, then the Zetetic philosophers are right. Dr. Hall can not bring up any passages from the Bible that directly sustain him. He claims only that certain inferences from it sustain him. Indeed, the bald literal sense contradicts him. One of his objections against the current way of speaking of gravitation is, that science has thus really made it nothing. Well Job says:

"He stretchest out the north over the empty place, and hangeth the earth upon nothing" (xxvi. 7).

Rev. Thomas Munnell says:

"The wave-theory must die; substantialism must live, because it is helping to bring life and immortality to light."—*Microcosm,* Vol. IV., p. 43.

The zeal of the adherents of each doctrine is of religious, not scientific, origin.

(2) *Both these theories are ingeniously defended, and are made quite plausible.* Dr. Hall, in opposing Zetetism, says:

"We thus speak of the founder of this doctrine because his book, false as his book evidently is, evinces no common ability or learning. The book, which is an exhaustive treatise on the subject, contains more than one hundred engravings of fine execution, making the entire work a masterpiece of scientific criticism and literary composition. Hence, in attempting to overturn a theory supported by such unquestioned ability and extended physical research, bearing on every proposition discussed, and aided by such a fund of general scientific knowledge as that author possessed, we are frank to confess that we had a much larger contract on our hands than we supposed at the start."—*Scientific Arena,* Sept, 1886, p. 57.

"Indeed, some of these books now in circulation are so puzzling, even to careful thinkers in matters of science, that their readers have often called our attention to the Zetetic system of astronomy, as it is termed by its advocates, and have urged us time and again to expose it in such concise and fitting terms as to place on record a definite and easily grasped antidote for this rapidly spreading fallacy of science."—*Scientific Arena,* August, 1866, p. 41.

(3) *Astronomers have let Zetetism alone, even as physicists have been indifferent to "substantialism."*

(4) *Both theories have illustrated the fact that it is*

very easy to confuse a man, however intelligent he may be, when he is on ground with which he is unacquainted.

"Finally, when two highly educated ministers of the city of Brooklyn, one a leading Episcopal clergyman, came to us and confessed themselves unable to answer the arguments of the flat philosophy, or to demonstrate the earth to be a globe in the face of its reasoning, we determined to commence a series of papers in the *Scientific Arena*," etc.—*Scientific Arena*, August, 1886, p. 41.

(5) *The advocates of both theories are violent in their abuse of "current science."* Both say scientific men of reputation are afraid to attack them.

"The more the followers of Copernicus laughed, the more industriously and audaciously would they circulate their books, and the bitterer would become their denunciation of the ignorance and prejudice of college professors in not condescending, or rather in not daring, to meet their invincible arguments, as they claimed them to be."—*Scientific Arena*, August, 1886, p. 41.

"Mr. Hampden complains bitterly that the scientists of England treat him and his Zetetic theory with contempt, paying no attention to his thousands of books, pamphlets and circulars sent out. They are called cowards and other bad names without stint, because they will not allow him to ventilate his views in their different scientific periodicals." —*Ibid*, p. 42.

With a slight, very slight, change of wording, these extracts can be applied to the "substantialists." I therefore adopt the Doctor's words as applying to him, and say, "Thou art the man."

(6) *Both finding that abuse had no effect, have occasionally tried flattery and soft words.* Mr. Carpenter dedi-

cated one of his books to Prof. Proctor, but that astronomer only wrote the author a note thanking him for the courtesy of the dedication.

Dr. Hall dedicates one of the later editions of his book "to the scientific investigators of Europe and America" with his "kindest wishes."

(7) *Neither Zetetism nor Substantialism is new.* The nature and the advocates of the one theory much resemble the nature and the advocates of the other.

In making this attack on physicists Dr. Hall tried to prove too much, and it is said that "Whatever proves too much proves nothing."

If Dr. Hall's arguments are founded on a true conception of the wave-theory, if they are all logical, and if all his conclusions are valid, then the scientific men of all ages have been a set of fools. It will not do for the Doctor, *when it suits his purpose*, to try to shift the absurdity from the adherents to the theory—it must be in both If Dr. Hall's arguments are valid, then the men who first formulated the wave-theory were idiots, and every man who has believed it since has been a fool. That is all there is of it, and without any mincing of words. He also tries to prove that in some things at least they knew they were teaching falsehood. That is, they were, and are, a set of *dishonest fools*.

In other walks of life scientific men have shown themselves to be the intellectual peers of any. They have blessed the world by their labors, and the world appreciates their work and is grateful. Thousands of them have been men of unblemished reputation. No man can be completely lacking in every element of common sense in one-half of his mind, and rotten to the core in the corresponding half of his character, and have in the other half of his mind a powerful intellect, and retain on the corresponding half of his character a spotless reputation.

Dr. Hall has always made "substantialism" of supreme, even exclusive, importance. Indeed, it is of more importance than the Bible! In the *first* article of the *first* volume of the *Microcosm,* the Doctor says:

> "*Instead of massing Scriptural proofs of the immortality of the soul,* which none question and few heed, let every clergyman in the land from this time forward boldly take the view of Nature here outlined, and maintain with incontrovertible proofs that the invisible and intangible 'forces' are as really substantial as are the corporeal bodies recognized by our senses, and, you may depend upon it, there will, at once, be opened to view a new world of substantial entities from which a flood of light will be poured into the skeptical mind."

This is in accord with the estimate he always places upon his theory.

Rev. J. I. Swander says:

> "Drs. Hall and Mott can never capture and hold the citadel of true philosophy and leave the wave-theory undemolished. As well might the great Captain of our salvation have tried to pass up, as our forerunner, to heaven with the keys of death and hell in his hand, and leave an uncaptured or unconquered grave in the midst of his triumphal march to the gates of the celestial city. If the Substantial Philosophy

> is true, the wave-theory of sound is false. If Substantialism is not true, Christianity has no durable foundation, and the fondest hopes of the human heart have no anchorage beyond the mutable stuff which materialism worships as the god of its idolatry."—*Scientific Arena*, June, 1886, p. 4.

Substantialists, like all persons of one idea, are fiercely intolerant. They allow no room for difference of opinion. In this same article, in speaking of Dr. Hall's book, Rev. Mr. Swander says:

> "Those who refuse to be convinced thereby would not believe, though one should arise and come to them from the materialistic hell."

Rev. F. Hamlin says:

> "No less patent is the error (and criminal, indeed, is the inconsistency and dishonor) of these men in still adhering to the 'wave-theory of sound' after the hot-shot from 'Wilford's' artillery have slain it beyond the possibility of a resurrection."—*Microcosm*. vol. i., p. 335.

The New York *Independent* spoke slightingly of Dr. Hall's book, and this rendered him furious. He closes one of his articles against that journal by quoting, with approval, these words of another:

> "Skinning skunks is not a very pleasant occupation, but we deem the work necessary in the case of Henry C. Bowen, and we have devoted some time and space of late to this work. What the skunk is to the lower order of animal creation, Mr. Bowen and the *Independent* are to journalism, and anything calculated to warn the public of the presence of the offensive animal we consider of essential value. The recent articles showing the true character of the *Independent* and Mr. Bowen have attracted a great deal of attention, not only outside but inside the *Independent* office."—*Microcosm*, vol. i., p. 106.

Dr. Hall is so egotistical that he supposed a word from him would crush this journal, but the *Independent* is more powerful to-day than when he made his assault.

Substantialism has wonderful powers of appropriation. A "substantialist" can claim that anything he chooses belongs to "substantialism." Seeing that the "new theology" is considerably older than "substantialism," one would suppose it would escape the latter's drag-net. But not so. In the midst of a long article on the subject, Rev. J. I. Swander exclaims:

> "Look out, therefore, brethren, lest by a similar Yankee trick the 'Problem of Human Life' should be burglarized for the purpose of robbing its immortal author of the glory which, under God, belongs to A. Wilford Hall. Some men will not advocate a newly-discovered principle of science until its leaven has begun to work in the public meal, and not even then unless they can see a chance to steal the product of another's brains, and shine in the questionable luster of another man's work."—*Scientific Arena*, August, 1886, p. 38.

Though "substantialism" has such powers of appropriation, Dr. Hall quickly resents any failure to give full credit to himself. See his sharp rebuke to Dr. Jacob Redding in the *Scientific Arena* for October, 1886.

I once heard an anecdote of an insane man whose hallucination led him to claim as his own personal property everything that pleased him. He was taken to the theater, but nothing on the stage seemed to interest him at all. At last there was some artificial thunder behind the scenes. Instantly his whole manner changed, and he sprang to his feet, exclaiming, "That 's my thunder!"

The "substantialists" can claim anything; but Dr. Hall does not allow anyone to steal his thunder.

The Doctor's vanity is astounding, and he takes the adulation of his followers as a matter of course. He has kept a column for such obsequiousness, headed "Kind Words never Die." The adulation at times approaches worship. His personality has utterly crushed out independence of thought among his followers. It never occurs to one of them to question or dispute anything he says.

At times the Doctor's vanity takes a comical turn. For example: A minister had, in a Thanksgiving sermon, made several references to Dr. Hall as one of the things for which we should be thankful. The Doctor quotes a long extract, in which he italicizes all the personal references, and nothing else. See *Microcosm*, Vol. II., pp. 206–7.

His admirers are all the time telling him of his "growing influence." I notice that the circulation of the *Microcosm*, at the end of its first year (or, rather, nine months), is stated at 50,000 copies (p. 357). The *Scientific Arena* succeeded this paper, and its circulation at the end of its first year, is given as 15,000 (June, 1886, p. 9).

When evidence becomes as substantial as this, "substantialists" do not believe in it. Besides, the idiom of our language allows us to say "growing" larger or "growing" smaller.

Just occasionally a "substantialist" gets a glimpse of the true state of affairs. Rev. J. I. Swander says:

> "It is feared that his [Dr. Hall's] recently projected hypotheses are nothing more than the beautiful dreams floating in the transient exuberance of chimerical speculation; that the new philosophy, around which so many hopes are clustered, will prove to be but an 'ill-guided bark, well built and tall, for angry waves to cast on desert shore.' The writer of this article is of the opinion that there is no occasion for any such gloomy forebodings."—*Microcosm*, Vol. II., p. 167.

The writings of Dr. Hall and his admirers are exceedingly intolerant in tone. The spirit in which "The Problem of Human Life" is written is abominable. Eliminate the abuse, and the volume would shrink to about one-half its present size. If the insinuations of dishonesty would then evaporate, and if the arguments should appear once, and but once, according to some logical arrangement, the book could be easily carried in the pocket.

When Dr. Hall was a young man, he was very fond of disputes on religion. At this time such disputes were almost universally bitter, often really dishonest. And this was true whether the discussion was between believers and unbelievers, or between persons of different denominations. Since that time the world has learned more of the spirit of Christ, and men are more tolerant of views differing from their own, if honestly held. The Doctor has resided many years in the metropolis of our country where many waves of thought meet and mingle. The spirit of Charity has been everywhere in the air, but he heard not her voice. He was in it, but not of it, and would, if he could, revive the spirit of other days.

"EVOLUTION OF SOUND" EVOLVED

CHAPTER I.

HISTORICAL.

"O, wad some power the giftie gie us,
To see oursel's as others see us!
It wad frae monie a blunder free us,
And foolish notion." —*Burns*.

In former ages Light, Heat, etc., were looked upon as substantial *entities*—a corpuscular theory for each. Dr. Hall does not champion new theories, but is trying to revive theories that the rest of the world has discarded. This fact is most carefully concealed by "substantialists" in all their writings. They heap abuse upon "so-called" scientists for never having been able to originate such a simple conception as that Heat, for example, is a substance. Their sentences are always so constructed as to leave the impression that "substantialism" gave this thought to the world within the last decade of years.

The fact is that this was the general opinion concerning the nature of Heat hundreds of years before Dr. Hall was born. An indefinite theory that "Heat is a mode of motion" can be found in the writings of the philosophers of all ages, but this was not the prevailing view. The generally accepted belief was that "Heat is substance or matter"—a substantial or material theory. Thus Prof. Tyndall says:

> Why should heat be generated by mechanical action, and what is the real nature of the agent thus generated? Two rival theories have been offered in answer to these questions. Till very lately, however, one of these—the *material*—had the greater number of adherents, being opposed by very few eminent men. Within certain limits this theory involved conceptions of a very simple kind, and this simplicity secured its general acceptance. The material theory supposes heat to be a kind of matter—a subtile fluid—stored up in the inter-atomic spaces of bodies.—*Heat as a Mode of Motion*, p. 37.

This book Dr. Hall has read, for he quotes from it, but I can not recall that he has ever quoted the passage I have just referred to, or the following:

> The laborious Gmelin, for example, in his Hand-book of Chemistry, defines heat to be that substance whose entrance into our bodies causes the sensation of warmth, and its egress the sensation of cold. He also speaks of heat combining with bodies as one ponderable substance does with another. And many other chemists treat the subject from the same point of view.—*Ibid.*, p. 38.

Prof. Tyndall discusses this view at some length. When a substantialist, however, refers to a late scientific book, he quotes only its definition that "Heat is a mode of motion," and, by omitting its statement that the prevailing view at one time was that "Heat is a substance," leaves the impression that scientific men always thought that "Heat is a mode of motion," that no one of them ever had any other conception. For example, Rev. F. Hamlin, D.D., says:

> It is true that some great names stand associated with the "mode of motion" theory. Aristotle considered heat to be "a condition of matter." Lord Bacon, in his *Novum Organum*, declares that "heat it-

> self, its essence and quiddity, is motion—motion and nothing else." Rumford said, in 1789, "It appears to me extremely difficult, if not quite impossible, to form any distinct idea of anything capable of being excited and communicated as heat is, except motion." Davy defines heat as "a peculiar motion," and in his "Chemical Philosophy" tells us that "the laws of its communication are precisely the same as the laws of the communication of motion." Locke, *later on*,* insists that "what in our sensation is heat, in the object is nothing but motion." And John Tyndall, in his "Heat as a Mode of Motion" (a work which has placed him in the front rank of "*so-called*" scientific expounders), holds the same view. Now, on the principle that every effect must have a cause, let us examine this question.—*Scientific Arena*, November, 1886, p. 85.

As we see, Rev. Mr. Hamlin had read Prof. Tyndall's "Heat as a Mode of Motion," but he so words his reference to it as to leave the impression that Prof. Tyndall had never known of any one holding the view that "Heat is a substance," and did not have the originality to conceive of such a thought himself. The minister of religion has a holy cause, let him not stoop to misrepresentation; yet, let this excuse be offered for Rev. Mr. Hamlin, he has but followed the code that most "substantialists" recognize as proper in discussion. A touch of candor in their writings is like an oasis in a desert.

Dr. Hall says emphatically that philosophers have never been able to grasp the thought that magnetism may be a substance. For example:

> Philosophers have been fearfully puzzled and agitated over these problems, and, though utterly unsatisfied with the meaningless phrase, "mode of motion," they have still clung to it as better than nothing, either not daring to strike out into a radical departure that would mean something real, or else not knowing what kind of a departure would best help them out of the difficulty. They have seen, for example, that

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* Davy was born in 1778; Locke was born in 1632.

magnetism would draw a bar of iron from a distance toward the poles of the magnet, and that it would thus act even through the most impervious of bodies, such as sheets of glass, as if nothing intervened. But they could not grasp the conception that this magnetic force was a real substance of some kind, because to them the insuperable difficulty of any kind of substance passing through glass as if it were not present, stood in the way. Hence, following those who have preceded them in such investigations, and chained to the text-books on the subject, they contented themselves with supposing it to be one of the mysterious forces of nature, and continued to view it under the general idea of some sort of *mode of motion.—Microcosm*, Vol. II., p. 52.

I turn to a Philosophy (third edition), published in 1838 by Prof. Denison Olmstead, of Yale College. It gives this formal definition of magnetism, and then discusses it:

> The phenomena of magnetism are explained on the hypothesis of two fluids, residing naturally in iron and all feruginous bodies, which, when united, neutralize each other's effects, but which, when separated, exhibit the respective effects of boreal and austral magnetism."—Vol. II., p. 192.

In this same article in *The Microcosm* Dr. Hall says that this theory concerning the nature of electricity—that it is a substance—is new. Prof. Olmsted says on this subject:

> There are some reasons derived from analogy for believing in the existence of an electric fluid. (1.) *The reasons in favor of supposing that light and heat are caused by the agency of peculiar fluids* (arguments, however, that we can not discuss here), *which have induced a general belief*, are for the most part equally applicable to electricity. (2.) In the present state of our knowledge, the most subtile of all fluids, indeed the most attenuated form of matter, is hydrogen gas, of which one hundred cubic inches weigh only two and a quarter grains, which is nearly fourteen times lighter than common air. But, at no distant period, means had not been devised by mankind for proving the materiality of common air, nor even of identifying the existence of other gases, which now bear so conspicuous a part in experimental philosophy. But, as knowledge and experimental researches have advanced, a series

of fluids still more subtile than air have come to light, until we have reached a body nearly fourteen times lighter than air, at which, at present, the series stops.

Is it probable, however, that nature stops in her process of attenuation precisely where, for want of more delicate instruments, or more refined and powerful organs of sensation, our methods of investigation and powers of discrimination come to their limit? An investigation of the general analogies of nature will lead us to think otherwise. The subordination which exists among the different classes of bodies that compose the other departments of nature is endless, or, at least, indefinite. In the animal creation, for example, beginning with the mammoth or elephant we descend, through numerous tribes, to the insect which is barely visible in the sunbeam. Before human ingenuity had devised means of aiding the powers of vision, the naturalist might have fixed this as the limit of animal creation. But the invention of the microscope has carried the range of human vision immeasurably farther, and, at each successive improvement in that instrument, new tribes of insects or animalcules have been revealed to the eye, still more and more attenuated. A similar subordination might be found in the vegetable kingdom, and in the organic structure of both animals and vegetables.

To apply this analogy to the case before us, we begin the series of organic bodies with platinum, and descend through bodies constantly diminishing in density, until we come to ether, the lightest of liquids, and on the confines which are invisible to the eye, and manifested only by the effects which they produce. By modern discoveries the series has been extended to hydrogen, a body two hundred and forty-seven times lighter than platinum.

Here, for the present, we pause, standing, in the same relation with respect to any fluids that may lie beyond that the ancients stood with respect to common air. Considerations of this kind lead us to believe that there are, in nature, fluids more subtile than hydrogen, and, such being the fact, we can hardly resist the belief that *heat*, *light* and *electricity* are bodies of this class—bodies which make themselves known to us by the most palpable and energetic effects, although their own constitution is too subtile and refined for our organs to recognize, or our instruments to identify them as material.—*Ibid.*, pp. 145–6.

This article anticipates almost everything that has been said by Dr. Hall on the nature of electricity, and also on the question of "Attenuation." If read to a "substantialist," he would indorse it until he found it was written almost half a century ago. Prof. Olmstead's book is still widely used in colleges, but all these views as to the nature of heat, light, electricity and magnetism are abandoned in the later editions. They would not stand the test of experiment. But Dr. Hall and his friends have taken up and vehemently advocate these views so discarded, and do not trouble themselves about any experiments. The belief that electricity is a substance has been abandoned; the tendency is toward looking upon it as a vibration, but no leading text-book that I know of commits itself to any view. Prof. Avery, who is about as definite as any, says:

> But little is known concerning the real nature of electricity. It is easier to tell what electricity can do than what it is. The majority of modern physicists consider that electricity is a form of energy producing peculiar phenomena; that it may be converted into other forms of energy, and that all other forms of energy may be converted into it. It is believed that electricity is a form of molecular motion, but this belief rests upon analogy rather than demonstration. Several theories have been advanced to account for electrical phenomena, but none of them is satisfactory.—*Elements of Natural Philosophy*, p. 192.

But we can not talk about a science without a nomenclature, and, generally, we can not have a nomenclature without a theory upon which to found it, and especially is this true in electricity. So, the books generally give a provisional theory for the sake of the nomenclature, generally called a "working hypothesis," and such a theory Prof. Avery gives. But concerning this he says, in his "Teacher's Hand-book" to accompany his Philosophy:

> The author simply prefers these theories to others with which he is familiar, he has no inclination in his own classes to insist upon them. His pupils understand that he holds them simply as hypotheses, scien-

> tific guesses, provisionally entertained for purposes of *mere convenience*, and that each of them has an equal right to make or adopt another, and to get others to do so if he can.—*Hand-book*, p. 73.

There is a striking contrast between the candid, charitable tone of an article written by a true scientist and the fierce, dogmatic tone of one written by a "substantialist."

The corpuscular theory of light is so well known that Dr. Hall and his admirers admit that they, in this case, did not originate their own theory, but that it is an abandoned theory revived.

I do not know that Dr. Hall ever claims that the theory of the dual nature of man—that he has a corporeal and an incorporeal body—originated with himself, but his references to that theory almost assume it if they do not assert it. For illustration, he says:

> When I first undertook the examination and review of Evolution, as taught in the writings of Darwin, Huxley and Haeckel, it became evident at a glance that the key to its overthrow lay in a single fundamental biological and psychological fact or law of nature, which had been totally overlooked by writers against the theory as well as for it, and that this oversight was the true cause of the failure to break through and satisfactorily explain the leading classes of arguments and facts relied upon in support of Evolution. This fundamental law of nature, susceptible of the clearest scientific demonstration, as shown in the eighth, ninth and tenth chapters, is that every animate being, including man, is a dual organism or double entity, the outer or physical structure being the visible half, while the incorporeal, though invisible and intangible, constitutes the other half, the one being the exact counterpart of the other.—p. 48.

His references to this theory always have this tone. The fact is, this theory is of German origin, though traces of it may be found in early Greek writings. It has been current in Germany quite a number of years, and has occasionally floated to our shores. Rev. Joseph

Cook delivered a lecture on this theory (though in a different nomenclature) in Boston, Dec. 25, 1876. He says he got it from the German, and, among the prominent men who advocate it, he names Prof. Helmholtz. The title of the lecture is, "Ulrici on the Spiritual Body," and it is reprinted in the volume of lectures entitled "Biology."

Dr. Hall emphatically states that a corpuscular theory of sound was unthought of among scientific men before it was advocated by himself. On this ground he thinks astronomers have done right in ignoring those who argue so strenuously in favor of the theory that the earth is flat, but says that physicists are not to be excused for ignoring his doctrine concerning the nature of sound, for it is new.

> But, unlike the crusade of Mr. Hampton, we think we have just ground of complaint at this silent indifference on the part of eminent investigators, since our facts, positions and arguments, whether correct or not, including the theory of "substantialism" growing out of them, were *entirely new to the world when first announced some seven or eight years ago.*
>
> They were not an exploded doctrine of centuries gone by, that had passed the ordeal of the most scrutinizing investigation and then been abandoned, nor had they been a represented theory, with the same stereotyped arguments slightly modified. Had we no better claims than this zetetic theory to a hearing, we would never think of blaming a scientist who had anything else in the world to occupy his time, for not giving the slightest heed to any such rehash of a long exploded theory."—*Scientific Arena*, August, 1886, p. 42.

Yet the fact is, the "corpuscular" theory of sound is older than the "wave" theory. Men at first thought that sound was a substance, that it was analogous to odor—the very same analogy that Dr. Hall is now advocating. On this subject Nicholson's Encyclopædia, published in 1818, says:

In the infancy of philosophy, sound was held to be a separate existence—it was conceived to be wafted through the air to our organs of hearing, which it was supposed to affect *in a manner resembling that in which our nostrils are affected when they give us the sensation of smell.* Yet, even in those early years of science, there were some, and, in particular, the celebrated founder of the Stoic school, who held that sound, that is, the cause of sound, was only the particular motion of external gross matter, propagated to the ear, and there that agitation of the organ, by which the soul is immediately affected with the sensation of sound."—*Art. Acoustics.*

It was natural that men, in the childhood of the human race, should look upon heat, light, sound, etc., as substances—the simplest of all conceptions. And, Dr. Hall, not deigning to profit by the experience and observations of men in all ages past, begins his philosophy just where humanity did, and thinks that all these things are substances, and if he should live two thousand years he will be just where humanity is and think they are "modes of motion."

But a book appeared in England a few years ago that is interesting in this connection. It is entitled, "An Elementary Hand-book of Physics," by William Rossiter, F. C. S., etc., published by William Blackwood & Sons, Edinburgh and London. The dedication of this work is dated November, 1870. The preface to Dr. Hall's book is dated June 1, 1877. The paragraph to which I refer in this English work has this heading, "*Nature of Sound.*" The title that Dr. Hall gives his monograph is "*The Nature of Sound.*"

It is easy to see that the corpuscular theory of sound which Prof. Rossiter is considering is not the same theory that Dr. Hall advocates, but the difference is slight. Prof. Rossiter says :

To show that sound is this alternate compression and expansion of successive spherical shells of air (supposing air to be the medium), and not a continual movement forward of any part of it, several illustrations must be given. Thus, to show that sound, like heat and light, is a transfer of motion, and not of matter, it might be enough to point out the many parallels in heat, light and electricity, the exceeding probability that sound is a phenomenon of like nature, and the absence of any facts to suggest any other theory.

Watching the tide rising or falling on the coast, it is difficult to realize that the waves are but the risings and fallings of the water, and not the transfer of some definite quantity of water from far off to the land. But careful notice of floating objects, such as pieces of wood, bottles, etc., shows that the water does but rise and fall. In the same way the passing of a brisk breeze over a field of corn illustrates the transfer of motion, but not of matter, very beautifully and completely. There is, however, this difference, that in the transfer of heat, light, sound, etc., the original impulse is given to the first wave only, and transmitted from wave to wave, while the wind that passes over a field of corn acts directly as well as mediately upon each stalk. But it is still certain that, however much appearances may suggest the idea of an actual transfer of matter across the field, it is but a transfer of motion, each part rising and falling in succession.

But it would be absurd to urge that sound is probably a vibration because heat and light are supposed to be vibrations, and that heat is supposed to be so because sound and light are, and, finally, that light is such because sound and heat are probably the same. It is necessary to prove the probability, if not the certainty, in one case at least, when we might be permitted to urge analogy as a reason, or to adduce satisfactory reasons in each case separately.

In the case of sound, it may be pointed out that the conditions of increased velocity and intensity are such as might be expected to obtain if the theory of vibrations were true, but are not such as fit a theory of emissions. Thus a sound is conveyed more rapidly through water than through air, and still more rapidly through iron, which could scarcely be the case if actual transfer of matter were required. Again, the intensity of a sound is increased by commencing in a dense atmosphere, which would retard rather than increase the force of matter actually in transmission. Also, any number of persons at the same distance from any source of sound hear it with equal intensity, whatever be their position, provided no obstacle intervene. This is what would be expected if sound be considered to be a vibration, but is not explicable upon the assumption that sound is an actual transfer of matter. Sounds travel many miles, and the size of a sphere, the radius of which is several miles, renders it scarcely possible that particles of matter proceeding from a central point could diverge so regularly as to form a spherical shell so large, or that particles so numerous could start from one point with such regularity as to form an unbroken shell of constantly increasing size.

We know that sound proceeds in all directions from its source, and that in the largest theater or church it never has been noticed that any two persons could hear while a third between them could not, unless, indeed, some obstacle intervened.

Then if it be imagined that sound is actually a transfer of matter, the question must occur, What is it that is transferred? I ring a bell in the middle of an empty room, so there are present but the bell and the air. Any hypothesis that the blow of the hammer upon the bell sent particles of the bell itself flying through space is scarcely worth discussion, especially when it is considered that the directions of sound seem in all cases to be quite independent of the law of gravitation, proceeding in unerring straight lines in every direction. And it seems almost as incredible that the particles of air in immediate contact with the bell should be set in motion for an indefinite distance through an atmosphere exactly like themselves in every respect.

Lastly, there is the fact that any number of particles, however great may be the number, is not indefinitely great, and can not fill up a constantly increasing space. Thus a slight stroke of a clapper upon a bell displaces a certain amount of air. This is made up of a certain definite number of particles of oxygen and of nitrogen. Assume these to be set in continuous motion, and not to be impeded in any way. By the surrounding atmosphere they could not increase in size, and so must separate, radiating from a common center, in a limited number of straight lines. These lines would diverge more and more from each other, so that only persons whose ears happened to be in the direction of one or other of them would have any perception of sound conveyed to them, while those between would be totally unaffected.

So that if a bell were rung in a large room, having a row of men standing ranged around against the wall, only one of every eight, ten or twenty, according to the size of the room, would hear it, while if the bell were slowly turned round while being struck, the sound would pass to each man in turn. Just as if a lantern, having but a few holes in its sides, were turned round and round, when the light-rays wou pass over each man in succession, and each would in succession be first lit up and then in darkness.

Therefore it seems very improbable, if not impossible, that sound can be either an emission of particles of matter, or the transmission of a few particles of air, while the theory of a transfer of vibration not only accounts for all the facts, but is in accordance with the calculations made upon data that force is indestructible and transferable.—pp. 23-4.

In reading the above, one can scarcely resist the impression that it was written in direct reply to Dr. Hall; it meets his arguments so perfectly. But this can not be; it was published several years before Dr. Hall's book appeared. If Prof. Rossiter had possessed Dr. Hall's volubility, he would have expanded that article into a volume.

If a man wants to believe and advocate theories that the rest of the world has outgrown, he certainly has the right to do so; but he should not try to palm them off as something new.

"Substantialism" is very old. In 1871 President Porter, of Yale College, said:

> Democritus was the first avowed *materialist*, resolving all the different kinds of being, with their phenomena, into combinations of atoms, differing in size and shape. He taught that *the soul differs from the body by being composed of finer particles*. All sense-perceptions are occasioned by contact. In modern phrase, he resolved all the senses into the sense of touch. . . . From Democritus, Epicurus borrowed the notion of *effluxes, simulucra rerum*, which he conceived in its grossest form, viz.: that they "*are like pellicles flying off from objects, and that these material likenesses, diffusing themselves everywhere*" *in the air, are propagated to the perceptive organs.—Elements of Intellectual Science*, p. 191.

Truthfully can Dr. Hall and his admirers sing—

"We think the same thoughts, we dream the same dreams,
We are the same that our fathers have been."

CLARENCE.

CHAPTER II.

THE PROPAGATION OF SOUND.

Suppose we have a rod three hundred feet long, perfectly straight, absolutely inflexible and incompressible, and resting in a groove.

A heavy iron mass, moving at the rate of five feet per second, strikes firmly against the end. How long will it take the shock to reach the other end?

Every thinking man will instantly answer: "To solve the problem so much data is not necessary. The disturbance will reach the other end instantaneously, whatever may be the length of the rod or the velocity of the impetus."

We change the conditions of the problem by substituting for the incompressible rod a coil or helix of wire. Now, how long will it take the movement to reach the other end? Our philosopher replies: "I can not tell exactly how long, but it will be some appreciable time. The mass of the coil has inertia which resists change of place of its particles. The consequence is that the end of the coil which receives the impulse is compressed, while the remainder of the helix is unchanged. But this leaves the coil with its forces unbalanced, and the wave of the compression runs along the coil until it reaches the other end."

I have seen a coil of wire six or eight feet long with one end fastened to a box, the other to be held in the hand. If, with a pair of pinchers, one compressed a short section of the coil near the hand, and then released it, a wave would be seen to run along the helix and strike the box, then be reflected to the hand. This would be repeated three or four times. It would be felt as a jarring of the hand, and be heard as a sharp click against the box.

The rapidity with which the impulse travels forward depends on two things:

1. The elastic strength of the spring.
2. The weight of the spring.

We see these principles illustrated in any spring of any kind. Suppose we have a long, slender steel rod firmly clasped at one end in a vise, the other end being free. If the free end be drawn to one side and then released, the vibrations are rapid. Why? Because the moving force (the elasticity of the spring) is great, and the work to be done (moving the weight of the spring) is small. Now attach a pound weight to the free end of the spring and again let it go. The vibrations are slower. Why? Because the moving force (the elasticity of the same spring) is the same and the work to be done (moving weight of the spring plus one pound) is greater; therefore it takes more time. This is the principle of "loading" tuning-forks and tuning organ reeds.

If a lead rod be substituted for the spring, the motion will be very slight, its weight being great and the elasticity weak. So the velocity of the wave in the coil will vary with these two conditions.

In the case of the incompressible rod, while the *transference of the motion was instantaneous*, each individual *particle of the rod moved with the same velocity as the colliding body.*

In the case of the coil it is a more dificult matter to say just what the velocity of the individual particle is.

Just here is where Dr. Hall mistakes the nature of the question at issue. He insists that the prong of a tuning-fork can not send off waves of air having a velocity of over one thousand feet per second, while the prong itself has a velocity of only a few inches in the same time.

We have seen that a body having a velocity of only an inch an hour might give a shock that would move forward at the rate of a thousand miles an hour, or even faster, instantaneously, while each individual particle of the rod would have a velocity of only an inch an hour.

Yet he says:

> "The velocity of such waves can not, by any possibility, exceed the velocity of the moving prong which impels them."—p. 90.

If not, why not?

The Doctor has an axiom he applies to all such problems, and it instantly solves them. His axiom may be stated something like this: no body can give to another a swifter motion than it itself possesses.

Now, that is a common sense statement and should be applied in a common sense way.

If Dr. Hall were asked to apply it to the first case we have assumed, this would be his way. He first recites his axiom in a declamatory, indeed dramatic style. Then he says: It is the simplest of problems. A tyro in mathematics, a boy from the common schools can instantly solve it. It will take just as many seconds as the velocity in feet per second is contained in the whole distance expressed in feet; that is, 300 divided by 5 equals 60 seconds, or just one minute! Though the books do not state it in so many words, yet the implication is that the shock is transferred instantaneously!! As though any body could communicate a swifter motion than it itself possesses!!! And nothing but the fatal delusions of a preconceived theory could ever have led to such a blunder!!!! *

Though the analogue of the hypothetical case of the incompressible rod does not occur in Dr. Hall's book, yet the case of the helix is closely similar to the propagation of sound.

To see this clearly, let us look at it for a few minutes.

We will first define some terms as used by physicists.

Compressibility is that property of matter by virtue of which a body may be reduced in size.

Elasticity is that property of matter by virtue of which bodies resume their original form or size when that form or size has been changed by any external force.

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* Dr. Hall, like all adventurers, got some of his followers with astonishing ease. Thus, Capt. R. Kelso Carter says: "My astonishment increased until, after reading some sixteen pages carefully, I came to this simple statement on page 90: 'The velocity of such waves can not, by any possibility, exceed the velocity of the moving prongs which impel them.' My conversion was ***instantaneous and complete.***"—*Microcosm*, Vol. IV., p. 39.

Dr. Hall cordially welcomes a convert, and counts his advent a great victory that should startle the world, whether the convert knows anything about his new creed or not, even as he did not about the wave theory. Thus one enthusiastic disciple talks (*Microcosm*, Vol. III., p. 380) about corpuscular "***omissions,***" in which the Doctor gently corrects him. But it is all very well. The less a man knows about any theory, the more enthusiastic a "substantialist" is he.

A glass ball is only slightly compressible, but it is very elastic, for, if allowed to fall on a heavy glass plate, it will rebound almost to the height from which it fell. Its elastic force is great, for it requires considerable force to distort it.

A rubber ball is very compressible, and also very elastic. Its elastic force, however, is weak.

A mud ball is equally compressible, but it is only very slightly elastic.

A steel rod is only slightly compressible, but it is very elastic.

Water is also only slightly compressible, but it is very elastic—that is, it regains its former volume instantly when the pressure is removed.

Air is very compressible, and it is equally elastic.

Suppose the adjacent prongs of two tuning-forks to be firmly connected by a steel rod. Were the bar perfectly incompressible, any motion of one prong would be conveyed to the other instantly. But the bar is slightly compressible; hence a moment of time will be taken in its transmission.

If water could be enclosed in a tube, and thus made to form the connection, the transmission of the movement would not be instantaneous, for water is also slightly compressible.

If two tuning-forks be immersed in water we have this arrangement, the enclosing tube being the surrounding water, whose effect is to allow the wave of compression to spread, and thus to become of smaller amplitude, yet to travel with the same velocity; just as the circling wavelets, when a stone is thrown into the water, become of smaller amplitude, yet their velocity does not diminish.

If the forks be immersed in air we have a similar case, except that air does not resist compression as vigorously as water, hence the velocity of the transference will be slower.

And we accordingly find that water conducts sound more rapidly than air does.

Remembering these principles, we can make tolerably good forecast as to the rate at which various substances will conduct sound.

We must remember, however, in making such forecasts, that density as well as elasticity is involved.

A given force can not move a heavy body as fast as it can a light one ; hence, considering the elasticity of all gases to be the same, the velocity of sound will be swifter in the lighter gas. And this anticipation, we find by experiment, is correct.

In the light of these principles, a table of the velocity of sound in various media appears quite reasonable.

I do not mean to say there are no difficulties in the minutiæ of the argument. But to be able to make some forecasts is better than to be unable to make any. On Dr. Hall's theory no one can make any forecasts whatever. Indeed, the velocity of sound in various substances appears so unreasonable as to cast grave doubts on his theory. Is it not astonishing that a vibrating bell can throw particles of itself sixteen times as fast through iron as through air? And our astonishment becomes greater when we consider Dr. Hall's statement that sound is of a nature similar to odor. Has it never occurred to him that all liquids and solids appear to be almost, if not altogether, non-conductors of odor, but are the best conductors of sound? We would scarcely expect odor, a substantial emission, as all admit that it is, to make its way through these substances.

If the metal coil we have mentioned be heated, it has less elastic force, and the wave through it will travel more slowly, for the helix acts more sluggishly. If steam be heated, it offers more resistance to compression, and air acts in the same way; therefore if, in the case we have assumed, the air in the tube be heated, it offers more resistance to compression, consequently acts more vigorously, and the wave travels more rapidly. Thus we see the probable reason why sound travels the more rapidly the warmer the air.

As I have said, I do not claim that all the details of sound conduction can be explained. The true scientist is not ashamed to say: "I do not know." Some of the most essential points in his theory (as, for instance, the generation and conduction of corpuscles) the Doctor passes by with the recurring formula that it is done "according to laws peculiar to themselves, but not understood."

When a man asks so much indulgence for his own theory, he should not refuse even the least to others.

Dr. Hall says:

> "There is no way of creating alternate rarefactions and condensations in the same mass of air every time a wave passes *except by the same air-particles traveling back and forth the entire distance from rarefaction to condensation, and vice versa*, as the two change places."—p. 140. (Italics his.)

A great deal of Dr. Hall's argument is based on that statement. And what a foundation! Is a body not condensed until it is condensed one-half? If 1000 volumes be compressed to 999, is that not a condensation? Or must it be reduced to 500 volumes before we can say it is condensed? The amplitude measures the distance that a particle in a wave moves. The amplitude may be small, while the wave-length is great.*

This we shall see, on incontestible proof, is as true of transverse "water-waves" as of longitudinal "air-waves."

A tuning-fork of the pitch of middle C has a wave-length of about four feet, yet, even if there were no lost motion immediately at the fork, the amplitude of the air-wave could be little, if any, greater than the amplitude of the fork in its swing.

Physicists have always taught that the transference of sound causes but a minute quivering of the air. Take, for example, these words from Prof. Olmstead's "College Philosophy":

> "In water-waves we distinguish between the wave and the motion which forms the wave. So here, the wave-motion is totally different from the motion of the air itself. The wave, *i. e.*, the state of condensation and subsequent rarefaction, travels swiftly forward, but the masses of air, which suffer these condensations and rarefactions, simply tremble in the line of that motion."—p. 187.

Prof. Helmholtz, reviewing Lord Rayleigh's theory of sound, says:

> "The mechanical principles of *minute* oscillations are contained in the present volume and are developed in a greater generality than in any other book known to me."—*Nature*, Vol. XVII., p. 237.

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* It is not necessary to discuss this phase of the subject in relation to various media. But even on this subject others reason differently from Dr. Hall. Thus, Prof. Dolbear, of telephonic fame, in discussing his instrument, says: "The velocity of sound in iron is about sixteen times greater than it is in air, and consequently the sound wave which is one foot in length in air would be about sixteen feet long in iron, while the amplitude of motion of the molecules would be as much smaller as its relative density is greater, that is, the actual displacement of the molecules must be as many times less in air when the energy of motion is the same."—*Scientific American Supplement*, Nov. 22, 1879.

And it is said in Ganot's "Elementary Treatise on Physics":

> "The amplitude of oscillation which is required for the production of audible sounds is very small. Lord Rayleigh determined it in the case of waves due to a pipe which sounded the note F, and which could be heard at a distance of 820 meters. He found that the amplitude of the oscillation of these waves could not be greater than the one ten-millionth of a millimeter."—p. 207. (A millimeter is about the one twenty-fifth of an inch.)

To talk about the amplitude of sound being necessarily great, even, indeed, half the wave-length, is ridiculous.

We have seen that the velocity of a wave of condensation (or rarefaction, if the piston be pulled backward instead of pushed forward) is only indirectly involved in the question of how fast the disturbing body moves. The fact that a tuning-fork's prongs do not move as fast as the waves of air has been to Dr. Hall an insuperable objection to the accepted theory of sound. He has spent all his eloquence of exaggeration, quibbling and abuse upon it. But it is really a far greater objection to his own theory. The corpuscles in his own theory are really nothing but *projectiles*, and, of course, can not have a greater velocity than the body which gave them the push (tuning-fork prong). Can the ball move faster than the hand which threw it? We freely grant indulgence on minor points, but this is a too essential point to be passed over by simply saying it occurs "according to a law peculiar to themselves but not understood."

The fact that the velocity of the wave may be great, while that of the individual particle is small, relieves "wave theorists" of a great difficulty. The velocity of a wave of light in ether is about 186,000 miles per second, but the velocity of the particles that beat immediately upon the retina may be, doubtless is, small. Hence the retina is not injured.

Dr. Hall's theory requires that an incessant volley of pellets having the enormous velocity of 186,000 miles per second shall be fired against the retina. And I believe that the retina would be destroyed, however small the projectiles may be, and we shall see good reason for thinking that these vague corpuscles have some considerable size. Circumstances demand that the Doctor make them so.

Some persons may still insist that the velocity of the disturbing body will modify somewhat the velocity of the transference of the disturbance, and I am not sure but that something of this kind occurs in the propagation of sound.

Dr. Hall assumes it as an undisputed fact that all sounds, loud and soft, travel with the same velocity. The deviation, it is true, is not mentioned in the smaller works on physics, but almost all the larger works, and especially the technical magazines, mention it. Ganot says:

> "For the same reason the tune played by a band is heard at a great distance without alteration, except in intensity, which could not be the case if some sounds traveled more rapidly than others.
>
> "This can not, however, be admitted as universally true. Earnshaw, by a mathematical investigation of the laws of the propagation of sound, concludes that the velocity of a sound depends on its strength, and, accordingly, that a violent sound ought to be propagated with greater velocity than a gentler one. This conclusion is confirmed by an observation made by Capt. Parry on his Arctic expedition. During artillery practice it was found, by persons stationed at a considerable distance from the guns, that the report of the cannon was heard before the command of fire given by the officer. And, more recently,

Mallet made a series of experiments on the velocity with which sound is propagated in rocks, by observing the times which elapsed before blastings, made at Holyhead, were heard at a distance. He found that the larger the charge of gun-powder, and therefore the louder the report, the more rapid was the transmission. With a charge of 2,000 pounds of gun-powder the velocity was 967 feet in a second, while with a charge of 12,000 it was 1,210 in the same time."—p. 187.

Dr. Hall would probably explain the first experiment by saying the vocal organs added nothing to the atmosphere, while the cannon added an immense volume of gas. But this does not explain the phenomenon. There was no appreciable movement of the air forward, else the observers would have felt a strong wind. But, of course, they did not. Besides, the voice of the officer was still in the air when the cannon was fired, and if the gases generated in the explosion pushed the air along to any appreciable degree they also pushed along the sound that the air bore. You can not make the hindmost seat of a carriage overtake the front one by pushing the vehicle forward.

In *The American Journal of Science* for February, 1879, Mr. W. W. Jaques gives an account of some experiments of his with a cannon. He found that the velocity was somewhat swifter the greater the charge; also that the velocity was greater in front of the cannon than behind it. But the interesting part of the experiment is this: the time when the sound reached a given point was not recorded by ear, but by a series of membranes which were shaken in succession by the advancing concussion, and this movement was recorded by an electrical attachment.

Dr. Hall says:

"If they shall not yet be able to distinguish between these two distinct effects, let them try the experiment of burning a couple of barrels of powder, and observing the effect at two separate stations—distant say one and two miles—with suitable instruments for recording the two arrivals of both the condensed wave and the sound report, and I again predict and guarantee that they will have abundant reason for abandoning the wave theory of sound by learning, to their amazement, that near to the explosion the concussive shock will outstrip the sound, while at a sufficient distance from it the sound will arrive some seconds in advance of the concussion.

"I have thus ventured this scientific prediction in direct opposition to the universally accepted theory of sound, and in the face of the prevailing opinion of scientists in regard to the identity of the sound-pulse and the condensed atmospheric wave caused by the explosion. Should any scientific association consider this prediction of sufficient importance to waste a barrel or two of powder upon it, let them explode the former by exploding the latter, and should they be successful in doing it, no one will feel more gratified at the result that the writer."—p. 115.

Now, Dr. Hall would say (as the record was not made by ear) that Mr. Jaques did not record the time when the *sound* reached a given point, but when the *concussion* reached it. Then, according to Dr. Hall's philosophy, this concussion should have shown a steadily diminishing velocity.

Mr. Jaques thus sums up the results of his experiments:

"The experiment was many times repeated, with the membranes interchanged, with different velocities and parts of the chronograph cylinder, and with other precautions, to prevent possible errors, but always with the same result. It was found that immediately in the rear of the cannon the velocity of sound was less than at a distance, *but that going further and further from the cannon, the velocity of sound rose to a maximum considerably above the ordinary velocity, and then fell gradually to about the velocity usually received.* In order to determine whether the first low velocities were due, as was supposed, to the retarding influence of the bodily motion of the air around the cannon, it was pointed at right angles to the first position, when it was found that the maximum velocity came nearer to the cannon."

As we see, the experiment that Dr. Hall proposed was not tried, but in trying the one they did they incidentally exploded Dr. Hall's prophecy—the concussion did not have a diminishing velocity.

These experiments were tried several years ago, but no one has taken the trouble to call Dr. Hall's attention to them, and, as he says, he will be much "gratified" when he reads them.

He seems to be, however, much given, on all such occasions, to relieving his feelings by bursting into a symphony of abuse.

This "condensed air-wave" sometimes produces marked effect at a distance of many miles. Mr. Jaques has found that it travels at the same rate as sound. What is it for, anyhow?

Dr. Hall has a great deal to say about the *mobility* of the atmosphere. It is the key to every difficulty.

He declares that when the tuning-fork's prong moves forward, that the air in front simply moves around behind, and no push is given to the atmosphere.

Well, why should the air so move around behind? It undoubtedly does move around, but it is because it meets with resistance in front—the resistance of other air particles, Dr. Hall speaks almost as though the particles moved around of intelligent, voluntary, even perverse choice. The particle of air only followed "the line of least resistance." And before it moved around it gave a push to the particle immediately in front of it, so this particle with the next, and so on to an infinite distance. But the push would be getting weaker as the circle got larger, and the distance each particle would move (the amplitude) would get shorter and the sound softer. The mobility of the atmosphere doubtless causes loss of motion immediately at the sounding body, but it does not annul all motion at a distance. The mobility of the water causes loss of motion at the steamboat wheel, but it does not cause a loss of all of it, so the boat moves forward, yet not as fast as the circumference of the wheel indicates it should.

CLARENCE.

CHAPTER III.

RESONANCE.

In examining the phenomena of Resonance we shall have a good illustration of how over-weening egotism, like Dr. Hall's, may lead a man to put himself in ridiculous situations.

Dr. Hall first quotes Prof. Tyndall, as follows:

> "A series of tuning-forks stand before you, whose rates of vibration have been determined by the siren. This one, you will remember, vibrates 256 times in a second, the length of the sonorous wave which it produces being, therefore,* 4 feet 4 inches. The fork is now detached from its case, so that when it is struck against its pad you can hear it. I hold the vibrating fork over this glass jar *a b*, Fig. 87, 18 inches deep, but you still fail to hear the sound of the fork. Preserving the fork in its position, I pour water with the least possible noise into the jar. The column of air underneath the fork becomes shorter as the water rises. The sound, you observe, augments in intensity, and when it reaches a certain level it bursts forth with extraordinary power. . . . Experimenting thus, I find there is one particular length of the column of air which, when the fork is placed over it, produces a maximum augmentation of the sound. This reënforcement of the sound is called Resonance. . . . Our next question is, What

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* The wave-length may be found by dividing the velocity of sound by the vibrational number or vibration frequency of the sound.

is the length of the column of air that most powerfully resounds to this fork? By measurement with a two-foot rule I find it to be 13 inches. But the length of the wave emitted by the fork is 52 inches, hence the length of the column of air which resounds to the fork is equal to one-fourth of the length of the wave produced by the fork. This rule is general, and might be illustrated by any other of the forks instead of this one."—p. 277.

On this Dr. Hall remarks:

"Those having access to the *Lectures on Sound* will observe that the engraving represents a jar as having an expanded or bell-shaped mouth. This single fact is the key which unlocks the mystery and solves the whole problem, giving the true reason for Prof. Tyndall's trouble in a nutshell. In order to demonstrate the correctness of this solution of the difficulty, I had three jars made specially for the experiment, all of the same diameter and height—one straight from bottom to top, one with an expanding mouth, the expansion being about one-half the diameter of the jar and extending down a couple of inches, the third with the mouth contracted or drawn in about as much and in about the same proportion as the other was expanded.

"By means of a series of careful tests with the same fork—256 vibrations to the second—I found that while the straight jar invariably gave a resonant depth of 11¾ inches, the one with a bell-shaped mouth gave a depth of 12¼ inches, while the one with a contracted mouth gave a depth of 11¼ inches. The conclusion was thus scientifically reached that with the mouth of the jar sufficiently expanded, and carrying the expansion down a sufficient distance, a resonant depth of exactly 13 inches might be finally attained, and in this way the experiment could be made to harmonize precisely with the necessities of the wave theory, making 52 inches the wave-length instead of 47, as results, and must always result, from using an honest jar."—p. 278.

It is easy to see that the Doctor thinks he has made a discovery. He has found that the length of a resonant jar is less than one-fourth of the wave-length of the note to which it responds.

Dr. Hall frequently speaks of himself as unaccustomed, until he began his book, to experimenting in natural philosophy.

It may seem unkind to tell him that the discrepancy he speaks of so exultingly has been known a long time. Some persons may say that beginners should always be encouraged, and I admit the force of this argument, but my seeming lack of consideration can, I think, be easily pardoned, for Dr. Hall, by natural endowment, does not appear to lack self-confidence.

Physicists have always considered the phenomenon he discusses identical in principle with the operation of the organ pipe; that the laws of the one apply (with slight modification because of the mouth of the pipe) to the other. Because of the practical importance of the knowledge to organ builders the discrepancy has been studied more in organ pipes, yet it was known that it is found in resonant jars as well.

Prof. Avery thus explains the theory of the flute-mouth organ pipe:

> "The current (Fig. 1) from the bellows enters through *p*, passes into a small chamber, emerges through the narrow slit *i*, and escapes in puffs between *a* and *b*, the two lips of the mouth. The puffs are due to the fact that the air current from *i* strikes upon the beveled lip *a*, and breaks into a flutter. The puffing sound thus produced consists of a confused mixture of many faint sounds. The air column of the pipe can resound to only one of these tones. The resonance of the air column, brought about in this way, constitutes the tone of the pipe. We see, from the above, that it makes little difference how the pulses of sound are formed. A vibrating tuning-fork held at the mouth of a pipe of the same pitch is enough to make the pipe sound forth its tone. *The production of the tone is strictly analogous to the phenomenon mentioned in paragraph 513.*" [This paragraph is on *Resonance.*]—*Elements of Natural Philosophy*, p. 406.

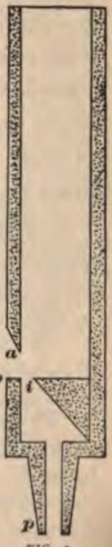

FIG. 1.

On this point likewise Prof. Tyndall says:

> "There are various ways of agitating the air at the end of pipes and tubes, so as to throw the air column within into vibration. In organ pipes this is done by throwing a thin sheet of air against a sharp edge. . . . Instead of a fluttering sheet of air a tuning-fork whose rate of vibration synchronizes with that of the organ pipe may be placed at the embouchure."—*Lectures on Sound*, p. 211.

The statement of these authors that a tuning-fork held to the mouth of an organ pipe of the same pitch will cause it to speak is only another way of saying that if a tuning-fork be held to the mouth of a jar of the right size it will cause it to resound. So all physicists speak of the resonant jar and organ pipe as similar in principle, and if the laws of one are given it is to be understood that they apply to the other also.

About all the smaller works on Natural Philosophy say the length of both a resonant jar and organ pipe (one closed at one end as the jar is) is one-fourth the wave-length for that tone. But when we turn to the larger works we find they say the diameter of the pipe as well as its length has its influence on the pitch. Thus Prof. Silliman says:

> "Practically the end of the tube where the sound is formed is never entirely closed, and usually the embouchure is on one side of the tube. The laws of Bernoulli adapted to these conditions give, for the construction of musical instruments, the following practical rules:
>
> "1. The length of the tube is equal to the quotient of the velocity of sound divided by the number of vibrations and diminished by *twice the breadth of the tube.*
>
> "2. The number of vibrations is equal to the quotient of the velocity of sound divided by the length of the tube *increased by twice its breadth.*
>
> "3. The length of a cylindrical organ tube, flattened at the embouchure, is equal to the quotient of the velocity of sound divided by the number of vibrations, and *diminished by five-thirds of the diameter of the tube.*"—*Principles of Physics*, p. 280, edition of 1860.

Thus Prof. Silliman always takes into consideration the *diameter* of the tube. But this knowledge is not confined to our own country, nor to the English-speaking nations. In a well-known German work I find the following statement:

"The conclusion resulting from the preceding paragraphs, that the pitch of a pipe is determined through the length only, is indeed not absolutely true, for all forms of the pipe, as the depth* of the pipe, the width of the mouth, etc., are of essential influence on the pitch.

"Savart has shown that two pipes *a* and *b* (Fig. 2), which have equal length and depth, but which are unequally wide, give the same sound, on the supposition that the mouth in both has the full width of the pipes, only that the sound in the narrower pipe is fainter.

"But when with equal height and width the depth of two pipes is different, as in the pipes *b* and *c*, then will the sound no longer be the same, it will be higher in the pipe of less depth, that is, higher for the pipe *c* than for *b*.

"According to Bertch, the pitch of two pipes is the same when the sum of the heighth and double the depth is the same for both; accordingly, a pipe of 10 inches length and 1 inch depth should have the same sound as a pipe of 8 inches length and 2 inches depth.

"When two organ pipes are similar, *i. e.*, when the corresponding dimensions are in the same ratio, and when the mouth is of proportional magnitude, and occupies the same position in both, then are the number of vibrations inversely as the corresponding dimensions. A pipe *a* will give a sound which will be an octave lower than the sound given by a pipe *b*, when *a* is twice as long, twice as wide, and twice as deep as *b*.

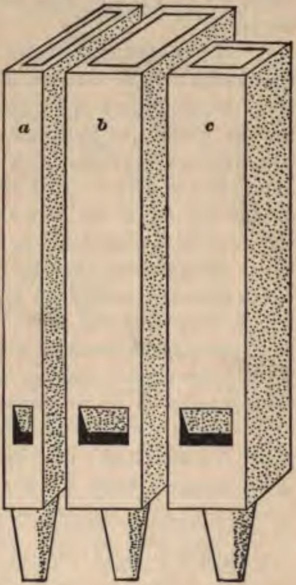

FIG. 2.

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* That is from the front to the back part.

"The magnitude and position of the mouth has a very considerable influence on the pitch of the pipe. It has already been remarked that when the width of the lips, that is, between the lips, is increased, it more easily gives its fundamental, but that it gives more easily its overtones when the mouth is narrowed. A different influence is exerted by the breadth of the mouth. When, in a square pipe, the mouth has the entire breadth of a side a higher sound is obtained than would be if the mouth were narrower. In this way the sound can be forced down even to the septime, especially when the pipe is almost cubic. For this reason organ builders attach small pieces of lead to the sides of the mouth hole, which are called *ears*, and which, by bending them, can be brought a little nearer to or farther from each other in order to regulate the pitch.

"It has been long known, through repeated experiments, that the sound of a horn or trumpet depends upon the material of the instrument and the degree of hardness. A horn which has been hardened in the fire without its form being changed would give only dull sounds. The organ manufacturers also know the influence of the material on the nature of the sound. And they assure us that it is but necessary to change only a little the nature of the tin in the metal pipes, or that of the wood in the wood pipes, in order to make the instrument poor. These observations have been confirmed by numerous experiments which Savart performed on pipes of more or less stretched parchment, and more or less moist paper. He found that the sound in a square pipe, the side of which is nine lines and the height one foot, can be pressed down more than an octave, when the paper which forms the walls is more or less moistened. This paper had been pasted on the firm edges of the prism as on a frame.

"He also found that by these means the sound can be pressed down the more easily the shorter the pipes are. Cubic pipes can be tuned down more than two octaves. Moreover, he found that but part of the wall need be made of paper in order to lower the sound." (Translation.)—*Miller's Physics and Meteorology*, Vol. I., p. 399, edition of 1863.

Dr. Hall discovered two things. One is that the length of a straight resonant jar is shorter than one-fourth the wave-length of its tone. The other is that if the jar is flared the depth is greater.

He seems to have been so delighted with his discoveries that he had no thought of pursuing the investigation any further.

Indeed, he did not try to explore what he supposed was an unknown country. He but touched the frontier and was satisfied. It is perhaps just as well, it is not likely he would have added anything to the sum of human knowledge on the subject.

The reason generally given by physicists for the discrepancy that Dr. Hall discusses is that the vibrating column of air can not take the same density as the external air precisely at the top of the pipe or jar, but that this occurs some distance above the top, hence the "measured" length (to use their phraseology) is less than the "theoretical" length. Also, this point is higher up the wider the jar, hence the discrepancy is greater the wider the jar or pipe.

Before giving any data on the question I desire to say a word about the phenomenon itself.

Dr. Hall speaks of this point of the greatest reënforcement of sound as though its position could be accurately determined. Capt. R. Kelso Carter gives, in the first volume of the *Microcosm*, the results of a large number of measurements he made with different forks. He says he took but one observation and measured to the fiftieth of an inch. Now, all this appearance of accuracy is misleading. If Capt. Carter had taken the measurements again he would have obtained results differing from the first by an eighth or a quarter of an inch, or even more. I know this by experience, and I have never seen any one who could obtain more nearly uniform results. As one nears the point of greatest resonance he notices that the sound is growing louder, when he has passed the point the sound again diminishes, but it is impossible for any one to decide positively as to the exact position where it was loudest. Also, it is impossible to make a tuning-fork always sound with the same intensity, and this deceives the ear. Hence the only thing left us is to take the average of several observations, and even then there is no dependence to be placed on small fractions. The edge of a rainbow is so poorly defined that two successive measurements of its width would not agree, or, if so, it would be accidental.

In giving data I have taken the average of at least three observations.

The fork I used makes 256 vibrations per second. The wave-length, then, at 60° F. is about 52 inches, consequently the "theoretical" length of a resonant jar is about 13 inches. The following are the result of some trials of the law:

DIAMETER OF JAR.	DEPTH OF GREATEST RESONANCE.
1¾ inches.	11 5-16 inches.
1¼ "	11 5-6 "
11-16 of an inch.	12⅓ "

Thus we see that the smaller the tube the more nearly does the "measured" length agree with the "theoretical" length, until in a capillary tube we would expect them to agree. In this case, however, the amount of air inclosed within the tube would be too small to give any appreciable resonance.

But this is but introductory to the argument that I am about to present.

If the explanation of the reënforcement of sound as generally given by physicists is correct, then we would expect this reënforcement to occur again at some other length.

Prof. Gage thus states the current explanation of resonance at one-fourth the wave-length :

> "How is this reënforcement effected ? When the prong *a* moves from one extremity of its arc *a′* to the other *a″*, it sends a condensation down the tube, this condensation striking the surface of the water, is reflected by it up the tube. Now suppose that the front of this reflected condensation should just reach the prong at the instant it started on its retreat from *a″* to *a′*, then the reflected condensation will conspire with the condensation formed by the prong in its retreat to make a greater condensation in the air outside the tube. Again, the retreat of the prong from *a″* to *a′* produces in its rear a rarefaction, which also runs down the tube, is reflected, and will reach the prong at the instant it is about to return from *a′* to *a″*, and, to cause a rarefaction in its rear, these two rarefactions moving in the same direction conspire to produce an intensified rarefaction. The original sounds thus combine with their echoes to produce resonance, but this can only happen when like parts of each wave combine with each, for if the tube were somewhat longer or shorter than it is, it is plain that condensation would meet rarefaction in the tube, and tend to destroy one another."—*Elements of Physics*, p. 291.

Thus we see that Prof. Gage assumes that the wave will move faster than the fork—a thing that physicists have always thought. Dr. Hall has drawn on his imagination for a vivid picture of the consternation that would be felt in the ranks of physicists when they learned, for the first time, this fact from his book. Now, they have always known it, and the thought did not frighten them a bit. Thus Prof. Tyndall says :

> "The motion of the wave, then, is *vastly greater* than that of the fork."—*Lectures on Sound*, p. 202.

In the vista of the last few months I see myself wading through page after page of reiterated statement that Prof. Tyndall never thought of this.

If the explanation given by Prof. Gage be carefully examined it will be seen to apply as well to the three-fourths of the wave-length as to the one-fourth. The reflected wave from the first node (as we may, for convenience, call it) coincides with the motion of the fork in its retreat and augments its sound, having passed over one-fourth the wave-length twice, or one-half the wave-length, the fork in the meantime having made one-half its complete swing and is just starting back. Then, the wave from the third node would catch the fork, not on *that* return, but on the *next*, for, from the first node to the third is one-half a wave-length, and the wave has passed over that distance twice, or just one wave-length, and the fork meanwhile has made just one vibration.

To generalize the argument, we would expect a reëenforcement of the sound to occur at every odd multiple of one-fourth the wave-length.

By experiment I found this to be true as far as I had tubes to carry out the test; the intensity of the sound, however, diminished as the length of the tube increased.

The fact that by thus reasoning on the wave theory results may be foretold is strongly confirmatory of that theory. It is absolutely impossible, on Dr. Hall's theory, to forecast results.

I have said that physicists explain the discrepancy between the "measured" and the "theoretical" length of organ pipes and resonant jars by saying that the air does not take the density of the surrounding air exactly at the top of the pipe or jar, but at some distance above it, and that this point determines the real length of the vibrating column. Naturally this point is pushed higher up the wider the jar, though experiment shows there seems to be a limit beyond which no increase of width modifies the discrepancy.

It seemed to me that this explanation, if correct, might be made to subject itself to experiment. These reënforcements of sound at different points achieve the object beautifully. If a small ring, covered with paper upon which sand has been sprinkled, be suspended horizontally in an organ pipe which is sounding, it is found that there are certain spaces where the air is in motion (as shown by the sand dancing), technically called "ventral segments;" and certain points where the air is not in motion (as shown by the sand remaining quiet)—these points are technically called "nodes." If there is a node at the first quarter, there will be two more, dividing the pipe into four sections. The fact that resonance is heard at the end of the first and third quarter of a jar is enough for my argument. The accepted theory says that a wave from the second node, should this be made a place of no vibration by the presence of water, would not get back in a proper time to cause resonance.

The first quarter, or ventral segment, is shortened by the open end. But this difficulty does not occur with the other three, hence we would expect them to have the full "theoretical" length. If we measure the distance between the first reënforcement of sound and the next, that is, between the first and third nodes, and divide this by two, we will get the length of each of the middle sections.

The wave-length of a fork giving 256 vibrations per second at 60° F. is 52.5 inches. Then the "theoretical" length of a resonant jar which will resound to it is 13.1 inches. I took a tube eleven-sixteenths of an inch in diameter. The first point of resonance was found at a depth of 12⅓ inches, the next at a depth of 38.⅓* inches. The difference between these, divided by two, will give the length of the second and the third section, that is 13 inches. The experiment was tried several times with similar results, sometimes, by accident, the exact theoretical amount was obtained.

When we consider the uncertainty that must always surround such measurements, the point of greatest resonance not being clearly defined, and the variation from the theoretical length of 13.1 inches that would have been caused by any deviation of the temperature from 60°, this experiment is seen to confirm the wave theory in the most emphatic manner.

We have seen that the first vibrating section in a long resonant tube is shortened by the open end. Experiment shows that the internal sections are not so shortened.

We also know that the first section is shortened the more the wider the jar.

But, while the first section is shortened the more the wider the jar, we would not expect the diameter of the tube to change the length of the internal sections. On trial I found that this anticipation is correct. After the first one, the sections (or ventral segments) of a column of air are of the same length, whatever the diameter. This clinches the argument. Thus we have another illustration of the fact that superficial observations, like Dr. Hall's, are often misleading.

If the top of the jar be bell-shaped it allows the air to obtain the density of the outside air more nearly at the top of the jar, hence the column *within* the jar is longer.

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*This is about an inch shorter than three-fourths the wave-length; as the -tening of the first segment lifted the first node, so it lifted the third node also.

If the top be contracted it has the opposite effect, seeming to obstruct the formation of equilibrium.

When both these methods of shortening the jar are employed we have the "Helmholtz Resonator," about which Dr. Hall talks so glibly and noticed so little. These are generally globular, sometimes cylindrical, vessels with a small hole in one end to be presented to the ear, and a large hole in the other end to be presented to the sounding body. By their wide form and contracted mouth the laws of resonance are completely disguised. Their length may be much less than one-fourth the wave-length. Surely Dr. Hall has never noticed this or he would have instantly seized upon it, expatiated upon it for a dozen pages, and ended by again declaring that "the wave theory has hopelessly broken down."

The phenomenon of Resonance demands explanation. The wave theory has a very satisfactory one to offer. The more it is studied the more conclusive it becomes.

Supposing that by his superficial observations he had overthrown that explanation, the Doctor proceeds to pronounce a panegyric on himself.

Does Dr. Hall offer any explanation of his own? No, unless we call a few words of his on the subject an explanation—words that in their intangible nothingness, and bordering on the supernatural, would have done honor to Astrology in its palmiest days.

The Doctor sees the weakness of his own explanation, and tries to divert attention from it by a harangue on the failure of the wave theory to explain it. He is like the cuttle-fish, which ejects ink into the water and then tries to escape in the darkness it has itself created.

In the first volume of the *Microcosm*, Capt. R. Kelso Carter gives the results of some experiments with a large number of forks. He found the jar always *too short*, and is rendered frantic with delight.[*] It does not appear that he thought the constant appearance of the reënforcement of the sound was a phenomenon that needed any explanation, or that there was anything remarkable in its occurrence. To him the fact that it followed so nearly as the wave theory requires suggested nothing. It does not seem to have occurred to him that a discrepancy thus always *appearing* and always *minus* was probably following some law itself. It is impossible for me to understand how any man who can see *discrepancies* so plainly can be so utterly unable to see *coincidences* at all.

At one time the results did not suit him, and instantly the satellite imitates the example of his primary and throws out unmanly insinuations. He says:

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[*] The fountain from which Capt. Carter drew his joy was this He held various tuning-forks over jars, and found the depth of greatest resonance. He multiplied this by four to get the wave-length, and this by the number of vibrations the fork made per second to get the velocity of sound. In each case the result was less than that found by direct trial, and he calls on the whole world to rejoice with him over the fact. Any one can see, on theoretical grounds, that this should be so. Concerning this calculation Prof. Tyndall remarks that its results are only "approximate." He says:

> You are provided with a tuning-fork and a siren, and are required by means of these two instruments to determine the velocity of sound in air. To solve this problem, you lack, if anything, the mere power of manipulation which practice imparts. You would first determine, by means of the siren, the number of vibrations executed by the fork in a second, you would then determine the length of the column of air which resounds to the fork. This multiplied by four would give you, *approximately*, the wave-length of the fork, and the wave-length multiplied by the number of vibrations in a second would give you the velocity in a second." —*Lectures on Sound*, p. 205.

Thus we again see that Prof. Tyndall knew and says that the depth of greatest resonance is only "approximately" equal to one-fourth the wave-length.

> "The remarkable agreement between the velocities obtained from the *A* and *C* forks did not strike my attention till this moment. I do not pretend to account for it, although the fact that both forks were made at Prof. Mayer's factory may in some measure account for it. I mean *no unkind reflection* here. [That is very evident!] But the immense singularity of this whole experiment as performed by Tyndall and Mayer must be apparent to every one."—*Microcosm*, Vol. I., p. 249.

Several of his forks, it seems, were made by Prof. Mayer, and it is his boast that none of them gave the correct result as to the velocity of sound, hence that expression can scarcely be said to fulfill our ideal of that Charity that thinketh no evil. The principle by which Capt. Carter has judged, however, is founded on Dr. Hall's rule for deciding the credibility of testimony. When a statement in a scientific book *suits his purpose* the Doctor accepts it as true; when it does not suit him he declares it is a falsehood. Nothing can exceed the unreasoning faith with which he accepts the statements of scientific men when he thinks those statements can be used to bolster his own theory, yet he will reject as absolute falsehood a statement made by the same man and in the same book if that statement is against him. Yet the credibility of each statement may depend entirely on the writer's truthfulness.

The "substantialists," all at once, remembered that it was becoming in a set of men who aspired to be called scientists to discover at least a few facts. So it was heralded forth in the *Microcosm* that Capt. Carter and others would begin a series of experiments in Acoustics from which great things might be expected.

But I never learned that they discovered anything. Nature yields none of her secrets to him who goes at her in the vindictive spirit that these men did. She has treated them as she has Dr. Hall. Neglecting theories, it is impossible to point to a fact that he has added to our knowledge of sound. The world reserves its praises for those who discover facts, not for those who spend all their time abusing discovers and inventors.

The organ pipe conforms perfectly to the requirements of the wave theory, and in turn becomes a powerful advocate of that theory.

If the temperature of the air within the pipe be made warmer, the pitch of the pipe becomes higher; if it is cooled, the pitch is lowered. This is what we would expect on theoretical grounds. The pitch will depend on how many excursions the wave of air can make from one end of the tube to the other in a given time, say a second. If the pipe is made longer, of course the air wave can not make so many journeys back and forth, and the pitch will be lowered. We know that sound (that is, the air wave) does not travel as fast in cold air as in warm; hence, to chill the air in an organ pipe lowers its pitch—the speed of the air wave is retarded.

But this is not all. If, instead of chilling the air, we fill the pipe with a gas that conducts sound more slowly than air does, the pitch in this case also will be lowered. In each case the amount that the pipe will be lowered or heightened can be calculated, and experiment (when a test is possible) confirms the calculation. In this way the velocity of sound in many gases has been computed long before it has been found possible to test it.[*]

Can a man believe that all these coincidences are accidental? That they do not, in the least, confirm the wave theory? I answer, yes. Let a man believe that to admit that sound may be a vibration and not a substance is to yield the very foundation of his religious faith, and to acknowledge that his hope for immortality is but a feverish dream, then, that man will think that all these coincidences amount to nothing. Dr. Hall and his admirers vehemently declare this to be their belief, and act accordingly. With such a conviction in a man's heart it is not likely he will come to a theory in Physics with an unprejudiced mind, or that he will judge the question on its own merits, but his decision will be determined by data that is entirely irrelevant.

There are many ways of proving that the air in an *organ pipe* is in vibration. For example, it can be done in this way. As I have said, if a ring covered with paper upon which sand has been sprinkled be suspended in a horizontal position, and let down into an organ pipe that is sounding, the sand will dance about on the paper, except at the nodes; these movements being visible if the sides of the pipe are of glass.

The air in a *resonance jar* is also in vibration.

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[*] Concerning this question Prof. Tyndall says:

"We had in our first lecture a table of the velocities of sound in other gases than air. I am persuaded that you can tell me, after due reflection, how this table was constructed. It would only be necessary to find a series of organ pipes which, when filled with different gases, yield the same note, the lengths of these pipes would give the relative velocities of sound through gases. Thus we find the length of a pipe filled with hydrogen to be four times that of a pipe filled with oxygen, yielding the same note, and this would prove the velocity of sound in the former to be four times the velocity in the latter."—*Lectures on Sound*, p. 218.

As I have said, the jar may be much shorter than one-fourth the wave-length, if the mouth be contracted. Take a breaker jar of such a length, paste tightly-stretched paper over the top, and with a sharp knife carefully cut away the paper until it responds to one of the organ pipes. If sand be sprinkled on the paper it will be seen to dance about whenever that note is sounded, even when held at a considerable distance, showing that the air within the jar is in vibration, and that this is communicated to the paper.*

Dr. Hall admits that in the generation of sound there must be vibrations of something.

In the organ pipe it can be nothing but the vibrations of the air that produces the sound. It is found that all pipes of the same length and diameter produce sounds of the *same pitch*, whatever the material. The *quality*, however, differs with different materials, because the pipe itself is thrown into vibrations—vibrations that may be felt—and these produce overtones that modify the quality of the fundamental. As we would expect, these minute vibrations of the pipe are not the same for all materials. There is nothing about the pipe to vibrate but the parts of the *pipe* itself and the *air* within it.

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* Membranes of goldbeater's skin stretched upon rings are very sensitive to sounds. Concerning them Prof. Deschanel says:

"These light membranes are peculiarly susceptible of impressions from aerial vibrations. In Regnault's experiments, above cited, it was found that membranes were affected at distances greater than those at which sound was heard."—*Elementary Treatise on Natural Philosophy*, p. 889.

Dr. Hall says such membranes are not moved by the sound waves, but by the "incidental" air waves. He declares that these are unimportant and "meaningless." Dr. Hall must be convinced that these "incidental air waves" have done more harm than good in the world. They have been clogs in the wheels of Science. For thousands of years they have been masquerading as the real sound waves, and the personification has been so perfect that the whole world has been deceived!

But all of us believe that an organ pipe would produce sound if all of its parts were of the hardest steel and imbedded in solid masonry. The minute vibrations of the pipe might be stopped, but that would interfere with the overtones only. But there must be a vibration of something. He says:

> "Sound is undoubtedly generated by the vibratory motion of whatever instrument produces it."—p. 76.

We have seen that there are but two things about the pipe to vibrate—the pipe and the air. We believe that a pipe would sound if all its parts were absolutely fixed. Then it must be the vibrations of the air that generate the sound—air waves running in the tube. But Dr. Hall thinks it foolish to suppose that air waves have anything to do with the generation of sound. He says:

> "The truth is, whenever scientific investigators shall come to understand that *air waves have nothing to do with either the generation* or the propagation of sound, and that they are no more an essential part of these phenomena than are the incidental waves sent off by a steamboat's wheel an essential part of the boat's forward procession, the wave theory will be at once relegated to the limbo of exploded hypotheses, taking its place by the side of the Ptolemaic theory of astronomy, where it should have been consigned a thousand years ago."—p. 119.

This doctrine is vehemently insisted on throughout his book.

There are just two things about the pipe to vibrate—the pipe and the air. One could mechanically be prevented from vibrating, and the Doctor says the vibrations of the other can not generate sound. Then such a pipe should be dumb. Surely a theory of sound that leads us irresistibly to conclude that an organ pipe can produce no sound, or at least could not if all its parts were fixed, can not be the true theory of sound.

Dr. Hall's new *canon* in Acoustics is a ***cannon*** aimed directly at all wind instruments.

CLARENCE.

CHAPTER IV.

A REPLY.

In the April number of the *Scientific Arena* Dr. Hall comes forth in a bitter reply to my first article.

He ignores my remarks on the "incorporeal body," but says I am right in saying that material theories of Heat, Light, Magnetism, Electricity and Sound have not been unknown in the past, but, to state it in the famous words of the debating club, he insists that "the conflux of the argument does not subtend the analogy of the case."

He says others have advanced *material* theories to account for these things, he himself teaches *immaterial* theories to account for them.

I think the difference is immaterial, as I shall now proceed to show.

The Doctor says:

> "It [Substantialism] teaches that the substances of the universe, as above expressed, are naturally divisible into two main departments, namely, *material* and *immaterial*, which means nearly the same thing as *corporeal* and *incorporeal*."—*Scientific Arena*, April, 1887, p. 173.

When I read this I remembered the Doctor's having used the following words in his book:

> "It seems to me irrational, as well as unnecessary, to talk of *any essential or radical difference in the fundamental nature of material and immaterial substances*, and I have never seen a basic distinction satisfactorily defined."—p. 33.

As we see, he says there is no "essential or radical difference in the fundamental nature of material and immaterial substances," then what is the difference between a material and immaterial theory to account for any phenomenon?

We know some of the properties of the *material*, then we know some of the properties of the *immaterial*, for Dr. Hall says the two are alike.

The material has mass; then the immaterial has mass.

The material has weight; then the immaterial has weight.

Thus I reasoned when the article was written, and I can not yet see that it is such a reckless and egregious blunder as the Doctor claims it to be.

The best known analogy that Dr. Hall discusses in his book is that of the analogy between Odor and Sound.

He says:

> "Yet those miles of odor-surcharged atmosphere were filled with *substantial emissions*, as all science unites in assuring us, *though not so tenuous, probably, as sonorous substance*, yet sufficiently near it to cause the imagination to retire discomfited and confounded."—p. 136.

That is, he says, the difference between odorous and sonorous corpuscles is a difference in *degree of attenuation*, not in their fundamental nature.

Dr. Hall has this to say on the nature of Odor, quoting, as we see, with approval, the words of Prof. Tyndall:

> "A grain of musk, as I will show in the fifth chapter, will fill cubic miles of space with its peculiar *substantial* corpuscles, called odor, without reducing its weight perceptibly, tested by the most delicate balance, yet this fragrance is a real and even corporeal substance, as admitted by all authorities on the subject. Prof. Tyndall says:
>
> "'In the sense of touch the senses are moved by the contact of the body felt; *in the sense of smell*, they *are stirred by the infinitestimal particles of the odorous body;* in the sense of hearing they are shaken by the vibrations of the air.'—*Tyndall on Light*, p. 57.
>
> "Now, when cubic miles have thus passed off from a mass of musk no larger than an acorn, as the magnetic corpuscles pass away from the poles of a magnet, will any scientific investigator for a single moment contend that these 'infinitestimal particles of the odorous body' are annihilated because it is impossible to gather them up or to even recognize them after they have subsided?"—p. 40.

Prof. Tyndall does not mean to say that odor by this division into "infinitestimal particles" has lost any of its physical properties.

When the particles are collected together the mass is substance; it is matter. And when the particles are separated they must remain substance and matter, for in the division of the mass the particles have not changed their nature nor lost their properties.

A grain of musk in the massive state I can not but consider as matter, and I can see no reason for supposing that in the division it has changed from matter to that which is not matter; yet Dr. Hall says an odor corpuscle ls the analogue of a sound corpuscle.

Dr. Hall gives no hint that he thinks a body in becoming tenuous loses any of its physical properties. In the last quotation I made he falls back on the "indestructibility of matter." He also says odor is made up of "*substantial emanations*"—the very same words by which he defines sound.

> "Before stopping to quibble about the impossibility of sound being substantial emanations from its inconceivable tenuity, let us try to grasp the marvelous lesson taught by the fox and hound. Though the wind may blow across the trail, carrying off for hours the odorous clouds which have risen from the instantaneous impress of the feet upon the earth, filling thus, perhaps, vast areas along the trail with those magical atoms of perfume, exceeding possibly in extent many times the four square miles of air surcharged by the locust, yet sufficient odor remains, extending for rods on both sides of the trail, to enable the hound to pursue his distant game with infallible precision."—p. 136.

Less than a year ago he said:

> "If a dog has the ability to select and isolate one single form of odor from a hundred other almost exactly similar forms, with these various forms of smell intermingled in the most confused and tangled manner possible, as here shown, and, at the same time, with odor a real objective substance, as the whole scientific world admits, is it not reasonable to suppose that the well-known ability of a practiced ear to select and isolate one single tone from an orchestra of a hundred different instruments, must come in like manner from the substantial nature of sound? If not, then what sense or meaning can there be in the so-called analogies of nature?"—*Scientific Arena*, September, 1886, p. 52.

It is true that the whole scientific world admits that odor is "a real objective substance," meaning by this that it is real objective matter, not matter with all its physical properties withdrawn.

If "substantial corpuscles" have no physical properties the Doctor should never have brought the tenuity of odor into his book, and should never have mentioned odor as the analogue of sound. His so-called analogy is utterly misleading if, in separating, the particles lose all their physical properties.

The doctor will quote the scientific books as saying that odor is a substance, and then in his application use the word with his own peculiar views attached to it. This is a method of playing "fast and loose" with the words matter and substance that is not fair.

The reason that the Doctor can do this plausibly is that he now says substance is simply the broader term and includes matter.

It is true that a thing may belong to both a genus and a species at the same time, but it can not have properties and not have them at the same time.

I am not treating his "analogy" unfairly, nor trying to make it "go on all fours."

The difference between the two ways of looking at the tenuity of odor is wide and fundamental.

According to one view, his analogy between odor and sound is reasonable, or rather it teaches something.

According to the other view, it is impossible to find any analogy.

It is impossible to reason about the "substantial corpuscles" in Dr. Hall's philosophy, for they have neither mass nor weight, nor any other physical property.

Mere existence is the only property that, as far as I can see, he has left them.

But I do not know that mere existence makes an object either substance or matter. Hundreds of people believe in the existence of God who do not believe He is either substance or matter.

One of the principal arguments that Dr. Hall has urged against the current teachings of science in regard to the forces of nature is that those conceptions are intangible and really unthinkable.

I would ask, How much more thinkable are the Doctor's "substantial corpuscles"?

We can have no clear-cut conception of any object which has neither mass, weight nor size.

The word "substantial," in common parlance, has a clear and tangible meaning, and Dr. Hall puts it before the word "corpuscle," and innocently supposes that this makes the conception more tangible and thinkable, even after he has robbed the word "substance" of all meaning.

Dr. Hall has not a follower to-day who has a clearer conception of what a substantial corpuscle is, according to the new definition of substance, than the scientific world has of what gravitation is.

If Dr. Hall will let the analogy of odor alone his philosophy will remain somewhat tangible, but exceedingly vulnerable. If he repudiates that analogy he has stripped his philosophy of all that has made it attractive to men, namely, its supposed tangibility.

Yet this very tangibility and simplicity is the danger of Dr. Hall's philosophy, and he sees it.

I have not much faith in any philosophy that makes the universe of God as simple as a problem in common addition in arithmetic.

The only way that can be done is to leave out all the difficult parts. The mysterious, of which any candid man will acknowledge the universe is full, must be ignored.

Of late years Dr. Hall has been trying to inject into his philosophy what he had at first left out, and the necessary result has been that his philosophy is as complex, confusing and mysterious as any other philosophy of the universe.

The charm of its tangibility is gone. While the thing acted upon and the thing acting were both material, differing mainly in the degree of attenuation, we could understand it; but when the thing acting loses all thinkable properties, the philosophy itself is no longer in a region where we can think.

Take, for example, these words as expressing his later teachings:

> "The term *matter*, as thus viewed, only embraces a small portion of the substances of the universe, namely, those substances which are ponderable or otherwise susceptible of chemical or mechanical tests, or such as are absolutely limited by material conditions. The term *substance*, on the other hand, not only embraces all material things, however gross or tenuous, but it includes all immaterial things, or such imponderable entities as are not confined by material limits or conditions, and hence, such entities as can not be proved to exist by any chemical or mechanical test."—*Scientific Arena*, April, 1887, p. 173.

Thus we see that the Doctor is abandoning the clear, but contracted, views expressed in his book.

Once when he said that heat, sound, magnetism, etc., were substance he meant much the same as saying they were matter. Now he says they are not matter, and he has the right to change his mind, but it is amusing to hear him say that he never claimed they were matter.

In the revised edition of Dr. Hall's book, published in 1878, occurs this passage (footnote):

> "If light, heat, sound, gravitation, electricity and magnetism are all demonstrable *substantial entities*, or *each a different kind of attenuated matter*, instead of a meaningless 'mode of motion' without any substance to move, and if these various emanations can penetrate the densest bodies, each by laws peculiar to itself, some of them moving corporeal and ponderous objects in defiance of all intervening barriers, then what reasonable excuse can a thoughtful mind frame for ignoring the unseen hand of God because it is unseen, or disbelieving in its own substantial and immortal entity because intangible to the physical senses?"—p. 55.

This edition also contains the metrical arrangement of Dr. Hall's book. Let us examine some passages in it. For example, he says:

> "And if this be truly stated
> That there can be no such motion
> In the granules of the diamond
> As would be if undulations
> Should occur among its atoms,
> Then must rays of light be *substance,*
> *Or attenuated matter,*
> Passing through the very texture
> Of what constitutes the diamond
> Without waves or undulations,
> Friction, contact or displacement.
> That light may be such a substance
> As can pass through solid diamond,
> Or through any dense formation,
> Such as emerald or crystal,
> Without physical disturbance
> Or displacement of its atoms,
> Though beyond our comprehension,
> Is a mystery no greater
> Than that currents from a magnet,
> (Proved to be substantial fluid,
> Since they move corporeal bodies)
> Penetrate the densest metals,
> And the most imporous structures,
> Without friction or displacement;
> Or than this so-called ether
> With the nature of a 'solid,'
> More like 'jelly' than a fluid,
> As distinctly taught by Tyndall,
> Should thus freely wave in diamond
> Without sensible disturbance
> Of its molecules or atoms."—p. 60.

Here we have the phrase "substance or attenuated matter," and I innocently supposed that the phrase "attenuated matter" was used as a synonymous term, explaining what was meant by "substance." Dr. Hall would doubtless say it means no such thing, and that it means that one is entirely different from the other. The phrase occurs five times on fifteen consecutive pages, and every time I misunderstood it.

My only excuse is, that if that is what Dr. Hall meant to say, I did not think he would construct the sentence in just that way. I thought he had a clearer style than that.

The Doctor says that light is "material":

> "Not so, if light be substance,
> Or *material emanations*,
> Which might penetrate some bodies,
> While some others might resist it,
> As electrical discharges
> Freely penetrate some bodies,
> And refuse to pass through others,
> Which could not be predicated
> Of ethereal undulations,
> Since that hypothetic substance
> Is supposed to fill all bodies,
> Granite just the same as diamond."—p. 65.

The Doctor says that gravitation is as atomic as air, and I have always understood that air, though very tenuous, is material.

> "If the force of gravitation,
> Which draws bodies toward each other,
> *Be not formed of real atoms*,
> Or be not intrinsic substance,
> *Just as much as air or gases*,
> Then it is a name for nothing,
> Of which man can form an idea
> As an actuating power," etc.—p. 67.

The Doctor says that electricity is attenuated matter (that is, he says this if he uses the same grammar that the rest of us do) and that odor is a substance.

"And electrical discharges
Might, instead of being *substance*,
Or attenuated matter,
Prove but simple undulations,
Or a kind of mode of motion
Of some ethereal substance,
And thus triplicate the ethers,
Held within a solid texture
Of a bar of gold or silver
At one instant of duration.
Why, in fact, make out that fragrance
But another mode of motion,
Rather than a real *substance?*"—p. 66.

The Doctor says that mind is closely related to material objects.

"*Mind can therefore be but substance*
Of a finer grade of texture
Than the gross material objects
Of which the senses take cognition.

. . . .

Intellect is therefore matter
Of the most essential fineness
And tenuity, surpassing
Every possible conception
We can form of sensual objects,
Just as hydrogen is matter,
Though imponderable its atoms;
Just as interstellar ether,
Which all modern science tells us
Freely circulates in diamonds,
And through densest glass and crystal
By which light is undulated,
Is a true material substance,
More like 'jelly' than like gases,
More the nature of a 'solid'
Than of atmospheric substance,
As will soon be shown from Tyndall."—p. 49.

I will again refer to the common edition, which does not contain the metrical arrangement, and which is, I think, more widely distributed.

Physicists have always considered the luminiferous ether to be material, and to have material properties. Dr. Hall mentions this, yet he says that the substantial pulses of light in his theory are of like nature.

> "I have just had occasion to intimate light itself could just as well be supposed to radiate in the form of substantial waves or pulses, as first to ignore such a substance entirely, and then substitute *another material* (luminiferous ether) almost infinitely more difficult to accept." —p. 75.

The simple fact is this. Within the last few years the Doctor in his periodical writings has begun a system of "hedging," but he is exceedingly anxious to keep the knowledge of that fact from his followers. This frantic attempt to draw a broad line of distinction between substance and matter is not found in his book. In it, substance and matter are almost, if not exactly, the same thing.

Take the following extract for example. The first things he mentions are material; he says they are also substantial:

> "To the philosopher, however, who has grasped the true nature of *substance* in its various stages of existence from the tangible to the intangible, from the visible, ponderable, physical bodies around us—such as platinum, iron, water, wood, flesh, air, gas and odor, up to the intangible, invisible and incorporeal substances of electricity, magnetism, gravitation, etc.—to such a student of nature there would be no difficulty in comprehending the additional fact that the life, soul, mind or spirit of a living man constitutes an interior substantial entity, an invisible and incorporeal organism ramifying the physical structure, as real as is the visible and tangible organism it animates; the one the exact counterpart of the other, though the inner must be vastly the more important of the two, since it is the moving force or motive power through which alone any physical body is able to live, move or think." —p. 29.

If this passage does not teach that the difference between platinum and magnetism is a difference in the degree of attenuation, what does it teach? The heavier one is matter, then is not the lighter one matter also?

The Doctor sometimes speaks of invisibility almost as though it could be made the test of immateriality.

Dr. Hall is surprised that I should consider his doctrine of attenuation much the same as that advocated by Prof. Olmsted. I ask the reader to compare this passage with the one I quoted from Prof. Olmsted:

> "When the heaviest metal can be converted into a gas, many times lighter than our atmosphere, after which the present power of the chemist and mechanician is defied to reconvert it to a solid form, it ought to teach us that this gaseous substance, under the manipulation of still higher wisdom and greater chemical resources, might be made to reach a second state of attenuation or sublimation almost, if not quite, justifying our conception of an 'immaterial substance,' and that, too, without a transition much greater than its first change from metal to gas."—p. 33.

It is only when Dr. Hall wants to get out of a difficulty that he is so anxious to draw the line between matter and substance. At other times he does not think it important.

Within six months Rev. J. I. Swander published his "Text-book on Sound." It is said, on the title-page, to have been "carefully revised by Dr. Hall." On page 172 we find:

> "*Question 20.* On what analogical ground should sound be regarded as a substantial entity, and not as a mode of motion, as present science teaches?
>
> "*Answer.* On the analogical ground of the consistent and harmonious uniformity of Nature's laws and principles of proceeding. As heat, electricity, magnetism, etc.—all phenomena-producing causes in Nature—are among the self-evident, substantial or entitive forces of Nature, it would be incongruous and out of consistent harmony for

> sound and light, equally phenomena-producing causes, not likewise to be substantial forces or objective entities. And as *the senses of smell, taste and tactility can only be addressed, and their sensuous impression produced by substantial contact with these organs*, it would be entirely unwarranted to suppose an abrupt change to nonentitative motion in the sensations of hearing and sight."

The first part of this answer we may pass over, though some persons may say it "begs the question." It assumes that heat, magnetism, etc., are substantial—the very thing in dispute. But he assumes them to be "self-evident." Is that to be called "self-evident" which can gain the assent of comparatively few persons? His assumption can not be said to be evident, to say nothing of self-evident.

The axioms of Euclid are also said to be "self-evident," and have commanded the assent of the vast majority of minds to which they have been presented.

But in the latter part of that answer it is said that the sensuous impressions of smell, taste and tactility can only be aroused by "substantial contact with the organs," and hence, on "analogical ground," the senses of sight and hearing can only be aroused by "substantial contact." But in the senses of smell, taste and tactility "substantial contact," we can understand, it is the same as material contact; hence, in the senses of sight and hearing there must be "material contact."

These quotations might be multiplied indefinitely, but I will not weary the reader with more. Enough have been given, I think, to show why I treated the theories in "The Problem of Human Life" as essentially material. Yet Dr. Hall thinks it "astonishing" that I should view the subject in that light. Dr. Hall must, I think, be easily astonished.

Dr. Hall has, I think, learned more real science since he began to publish his periodical than he ever had learned before, and he has seen that, on experimental grounds, material theories of heat, magnetism, etc., are extremely unsatisfactory. So the Doctor quietly explained away all the material meanings that he had allowed the word "substance." The necessary result of this was that he left it no meaning at all.

The surprising thing about it is that he was able to do this without letting his followers know what was occurring. Dr. Hall certainly has the right to change his ground, but he can not be on both sides of the question at the same time.

I know of no intimation throughout all the Doctor's periodical writings that he desires to retract any leading, or even secondary, doctrine taught in his monograph on acoustics, or, using the name he generally applies to it now in the "Evolution of Sound." He always speaks of himself as willing and anxious to defend every doctrine taught in it. But when its material theories are mentioned he vehemently exclaims, "There are no material theories in the book."

In the light of the quotations I have given, and which might be greatly multiplied, this disclaimer is astounding. But it is the Doctor's business, not mine, to explain the grounds on which it is based.

He got most of his followers by appealing to the religious feeling, and offering, as he said, a tangible theory which all could understand.

A follower of Dr. Hall started out with a somewhat clear idea of what a "substantial corpuscle" was. But the refining away of all thinkable ideas as applied to substance has been so insidiously done that this man does not know that he has not the least conception of what a "substantial corpuscle" is as now spoken of. The word only is left him, and he repeats that to himself and innocently supposes that it represents some thought that is in his mind.

Every one knows that there has been a great falling away of the disciples of Dr. Hall. Very few of them, I think, went away because they understood the facts in the case, namely, that they understood they had been invited to a "substantial" feast—substantial in the sense that we all like substantial feasts—but when they sat down they found they were expected to dine on something as intangible as moonshine. The food looked substantial when they partook of it, but a craving for something really nourishing drove them unconsciously elsewhere.

"Substantialism" was a very good name for Dr. Hall's philosophy as he first advocated it; his present theory should, however, be called "Unsubstantialism."

Of course, I do not believe or say that that which is not matter has no existence. I am only insisting that our conception of that which is not material is exceeding nebulous—so nebulous that we can not reason about its nature.

In Dr. Hall's book substance is much the same as matter. In it "substantialism" is a tangible and thinkable philosophy. It was this that made it attractive to large numbers of intelligent though not highly educated men. But it was also a very vulnerable philosophy, and Dr. Hall came to see it was so. Consequently, of late, he has been refining away all the material meanings of the word "substance," until the word means nothing tangible, nothing thinkable.

Dr. Hall now virtually admits that he does not know what "force," for example, is; and that is what we all admit, only we do not try to deceive ourselves by putting a qualifying word before our ignorance (and saying "substantial force"), when we know that the qualifying word can have no thinkable meaning. It was one of his pet arguments that force had been treated by physicists as something unthinkable, and was consequently nothing. At first he allowed his conception of force to be thinkable—it was material—but its structure and composition is utterly mysterious to us according to the new definition of "substance." The word only is left, all its attributes are gone. I do not know another case where so large a body of men have deceived themselves so long with the mere ghost of a word—a word that once had a clear and living meaning to them.

Dr. Hall is surprised that this distinction he draws between the material and the immaterial does not strike the world as a wonderful accomplishment and produce an intellectual earthquake. He is quite sure it is nothing but bigotry that prevents the scientific world from hailing his views with inexpressible delight. He is so well pleased with that explanation that he does not consider it possible that there can be any other. But the distinction has been drawn before, but most people could not see that it really advanced their knowledge very much.

I have been examining the second edition of a book published in Boston, in 1858, by John P. Jewett & Co. The title-page reads, "Debt and Grace, as Related to the Doctrine of a Future Life, by C. F. Hudson." Let us make a few quotations:

"Our theory of the ubiquity of the atom is not refuted, we think, by saying that the atom attracts its fellows by a certain power of force. *Such power or force is either a thing or a nothing.* The distinction of substance and attribute does not meet the difficulty, for attribute is only a modification, quality or form of substance, and can not exist separate from substance. It inheres in substance; it can not exist without it."—p. 245.

"The vital or thinking principle perhaps must be an added *immaterial substance,* working upon, and with, and through the material, yet having its own proper and distinct being."—p. 246.

"If the human soul or spirit is not an *immaterial substance,* but perishes with the body, then the wicked will wholly die twice, and the penalty of the law will appear to be executed a second time."—p. 247.

"And we have set forth these marvels of the nature of matter, not to encourage the supposition that it may possess, or that it may be endowed with, the power of thinking, but for the opposite reason, viz.: to suggest to the materialist that there may be a *spiritual substance.* The subtlety of *matter* in its essence is hardly surpassed by the subtlety that is claimed for *spiritual substance.*"—p. 245.

Dr. Hall wrote a work that had the same theme as this book, namely, the destiny of the wicked. Mr. Hudson's conception of the nature of an "immaterial substance" was too intangible to make any deep impression. Dr. Hall's conception of its nature was quite tangible as he first advocated it. The difference between the material and the immaterial was principally a difference in the degree of attenuation. But he has been gradually refining away that view until his doctrine is as intangible as Mr. Hudson's.

When a principle of Nature is brought before our attention, the nature of which is utterly mysterious to us, let us not practice any self-deception by giving it a name that signifies that we do understand it. I doubt if "substantialism," as now advocated by Dr. Hall, can be either proven or disproven. It is in a field where we can not think.

Let us admit our own limitations, that there are fields of thought of which the mind can at times catch hazy glances, but it can not even cross the border line, much less traverse the broad domain. When man first began to study nature, he over-reached himself and attempted the impossible. For two or three thousand years men tried to find out what the forces of nature *are*, and learned nothing about them. For two or three hundred years men have been trying to find out what the forces of nature *can do*, and have learned a great deal about them. This about marks our limitations.

The answers of science are proximate, not ultimate. We see some phenomenon and say, "What produced it?" We study it and say gravitation caused it. But what caused gravitation? Science has no answer.

Our "cause" is a proximate, not an ultimate cause. Utter failure in the past in trying to find out the nature of ultimate causes gives no hope for the future. I do not believe we will ever understand what the forces of nature really are. Partial progress in the past in finding out proximate causes is an earnest of better things to come. We can find out much of what the forces of nature can do. But I have no desire to discuss the metaphysical side of "substantialism." I leave that to the metaphysicians, of whom I am not one.

But I am fully pursuaded that sound is neither substance nor matter. For this reason I was disappointed in that Dr. Hall declined to notice my second article, for it was on the question of sound. He quotes six lines from it, in which he says I have again confounded the material and the immaterial, and says this is reason enough for so declining. Seeing that those six lines are upon a secondary question that came up in the discussion, and have nothing to do with the main argument, his words look more like an excuse than a reason. The reason, however, that the Doctor did not desire to discuss that chapter is evident.

He had always said that all sounds, loud and soft, travel with the same velocity. A correspondent had just written him, quoting from a civil engineer who said that the velocities of all sounds were not the same. The Doctor was astonished that a civil engineer should make such a blunder in physics, and published a long editorial on the subject. Simultaneously with that article (in the March number) I published some of the data from which physicists have concluded that the intensity does influence the velocity. It was doubtless a new thing to Dr. Hall; yet he saw it was a simple thing in physics, and widely known. He thought that if he did not call attention to the fact by mentioning it few of his readers would know of his blunder.

In this same article he had again boasted of his prophecy that the condensed air wave caused by an explosion would be found by experiment to have a steadily diminishing velocity. I showed him that the results of some experiments tried several years ago in testing another question entirely, yet incidentally, disproved his prophecy. Dr. Hall did not desire to discuss this either. Skillful as he knows himself to be in explaining away difficulties, he doubtless thought that to succeed in explaining away this difficulty would do his cause no especial good, and to fail in the least would do him serious harm—it would shake the faith of his disciples in his infallibility—hence he just ignored the question.

MARCELLUS THOMPSON.

CHAPTER V.

"THE LOCUST ARGUMENT" EXAMINED.

There is a little instrument called the "lovers'" or "mechanical" telephone, which is of easy construction, is simple in principle, and is extremely interesting in the results that may be obtained with it. Its principle of action is entirely different from the telephone of Prof. Bell. There is no electrical current involved in its action.*

A rude instrument may be made as follows: Take two short cylinders of three inch bore, open at both ends. Small tin cans with the ends melted off will do.

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*The following quotation is a curiosity. The writer evidently supposes that Physicists teach that in the electrical telephone the air is condensed into little waves on the wire, and these travel forward! Prof. R. G. Hand says:

"But what about the submarine telephone? Do air waves cling to the outside of a protected or submarine wire where no room is left for such external physical encumbrance? If not, is the mode of transmission changed at every station, like passengers from stage to railroad, and *vice versa*, a few times on a journey? In other words, do air waves convey the messages from your mouth to the transmitter, then the diaphragm of the transmitter transfers them to the wire, which in turn transfers them to the receiver to be again transferred to the air, and finally to the ear-drum? Is there not danger of damage to delicate messages in so many changes of conveyances and transfers of freight?"—*Scientific Arena*, July, 1886, p. 20.

Over an open end of each stretch and tie tightly a piece of bladder. Pass a small firm cord through the middle of each bladder. To prevent its pulling out, tie a small flat button to each extremity of the string. When the tension is correctly adjusted, words spoken into one end can be heard at the other. The vibrations of the air cause one membrane to vibrate, this communicates its motion to the string, this, in turn, causes the other membrane to tremble, and this latter causes the air to vibrate in a manner similar to that of the original sound. This instrument is, of course, crude, and better results may be obtained with one more carefully made.

The form that I used is called "The Wilcox Screw-Tension Telephone." Metal diaphragms are substituted for the bladders, and a copper wire takes the place of the cord. It has an attachment by which the tension of the conductor may be changed. The wire must be kept taut. Though the lack of a bell is a disadvantage, yet the instrument becomes quite useful in some circumstances. Three hundred feet of wire are furnished, and the makers say more can be added if desired.

Standing six feet from the receiver, I have understood words spoken by a person standing one hundred yards away. Like all similar vibrating diaphragms, its words have a strongly nasal twang. The words are found to be somewhat more distinct if the string is stretched in a straight line; but the sound may easily be made to turn a corner by a simple mechanical device furnished with the instrument. Strict directions are given by the makers that the wire must not be allowed to touch any fixed support, but is to be suspended freely in the loops of a string. With a short wire, and the words spoken in a loud voice, the vibrations of the wire may be felt when it is grasped between the thumb and finger, but ordinarily the movement is imperceptible. Conceiving clearly in the mind the way the diaphragms vibrate, one instantly sees that the vibrations must be "longitudinal," not "transverse;" that is, any given particle of the wire must move *back and forth along* the line in which the wire is stretched *not across* it. This distinction is fundamental, and should be seen clearly. All sound waves are "longitudinal," water waves are "transverse."

As I have said, the makers say the wire must not be allowed to touch any projecting corner, yet if one allows it to so touch, he finds that the intensity of the sound is not greatly diminished. The sound is, however, so greatly confused as to be scarcely intelligible. The confusion may arise, in part, from the vibrations themselves becoming modified, but most of it, I think, arises from the friction of the wire against the support, the sound of which is carried to the diaphragms. But if, while sound is passing, the jaws of a clamp which is *firmly fixed* be allowed to close on the wire, its power to conduct sound is destroyed. A rope may be drawn back and forth through a hole which it fits neatly, but it can not be thrown into vertical or horizontal vibrations at that point. It is capable of "longitudinal" but not "transverse" vibrations. A mere projection would not stop longitudinal vibrations of a telephone wire; a clamp would stop both.

Before the exact tension is found, there is only a confused, metallic rattle of the diaphragms. The quality of the voice, though rendered nasal, is not destroyed, so that the speaker may be identified. The auditory nerve is placed in a cavity scooped out of the bones of the head. The teeth are firmly set in sockets in the bones—hence any vibration that may be communicated to them is quickly carried to the nerve. This is the principle of the "dentiphone." So here, if the wire be taken between the teeth the words are quite distinct. Words may be understood when the speaker is six feet from the instrument, though the sound is then faint; and the louder one speaks the louder is the sound given forth at the receiver.

Suppose that the speaker is standing three feet from the instrument. Then a great number of diaphragms might be placed three feet from the speaker's head, making a sphere six feet in diameter. A globe six feet in diameter has a surface of 16,200 square inches. The opening by which sound enters the instrument is circular in form, with an area of about one-fourth of a square inch. Consequently, 65,000 diaphragms might be so placed that one person speaking would set them all in vibration. But if the diaphragm of the transmitting instrument moves, the diaphragm of the corresponding receiver must also move, and also the connecting wire.

One hundred yards of wire weigh about ten ounces. Every instrument would set this mass in motion. Consequently, in 65,000 instruments 41,000 pounds would be set in motion, or over twenty tons. But it is evident, as the makers say, that the instrument can be used for much greater distances; and the energy involved would increase in the same ratio that the wire does.

If the wire is six hundred feet long, the energy is equivalent to causing vibrations in forty tons. If we "load" the wire it amounts to the same thing. Suppose we solder a block of metal to the wire. Then, if the sound passes, the block as well as the wire must move. So great a weight coming directly on the diaphragms might break them, but we can suspend it. I hung a little tin pail directly over the wire, and soldered this to the bottom. The instrument could still be operated. I placed a weight in the pail. Sound still passed, but with diminished intensity. As more weights were added, the sounds constantly grew weaker, until the weight amounted to four pounds, when the tone was very faint, and the experiment was carried no farther.

As the ten ounces were equivalent to twenty tons in the 65,000 diaphragms which one man could set in motion, so here these seventy-four ounces (sixty-four plus ten) are equivalent to 148 tons.

Every man who has no theory to uphold or oppose will admit that sound in this little instrument is conducted by vibrations. Else why should it stop the sound to allow a clamp to close upon the wire? It does not stop the water to grasp the tube through which water is flowing; and Dr. Hall says substantial sound is flowing through the wire. Also, on any theory except the vibrational, how can we explain the diminishing tone as the more weights were added? On that theory the explanation is easy. A given amount of energy can not move a large mass so fast or far as it can a small one. But I found that a loud sound could be forced through a "loaded" wire when a soft one would not pass.

Dr. Hall's locust can be heard a mile—a much stronger tone than the voice was in these suggestive experiments. Dr. Hall can calculate how much the wire might be loaded if a locust would only be kind enough to furnish one its delightful solos at the proper time and place. Of course the man is stronger than the locust, but the comparative strength of the two is not in the question. A man may take a sledge and strike a rock with his full strength, and not be heard very far. He may take a small hammer, strike a bell, and be heard much farther. In the second case he could convert more of his energy into sound vibrations. The fact that a locust can surpass a man in thus transforming its energy into sound is shown by the fact that it can make itself heard much farther.

Dr. Hall complains of scientists for talking about vibrations in air. He wants something more tangible, he says. So he invented "pulses of corpuscles," which are infinitely less tangible than air. I have taken something really tangible—masses of copper and iron.

If one takes a jar, pastes over it a piece of paper, then with a sharp knife carefully cuts away the paper until it is in unison with some tone, he will find that sand sprinkled upon the paper will dance about whenever that note is sounded, though the jar be at a considerable distance from the sounding body. Dr. Hall gives eighty feet as the distance to which it will respond to the voice. He says:

> "The sand dancing on a stretched membrane eighty feet away, by a tone of the voice, is not done by shaking the air at all. It results from a substantial sound-pulse striking a membrane in unison with its vibrational number, the tone thus exciting it into tremors by sonorous sympathy, as a magnet emits pulses of immaterial substance to excite sympathetically and move a piece of iron! [Exclamation mark his. He seems to be surprised at his own explanation.]—*Microcosm*, Vol. III., p. 369.

The Doctor insists that it is not the vibration of the air that causes the sand (and, of course, the membrane under it) to move. I think it is, but for the purposes of this particular discussion that is not important—the fact that the sand moves is enough.

If a jar in front of a person would respond, so would one at his side or behind him. A whole circle might be placed around him, and all would respond at the same time. It is not probable that the jars could be placed as far behind him as in front, so we take seventy feet instead of eighty as the radius of the circle. But another row might be placed just above this, and they would respond also, and so on until the entire dome should be filled.

The surface of a hemisphere of seventy feet radius is 440,000 square inches. If each jar takes up twenty square inches of this surface, 22,000 jars might be placed seventy feet from the speaker's head, all of which would respond when he sang the proper note. Suppose each membrane to weigh five grains and the sand upon it three grains, then eight grains are shaken on each jar. Then 22,000 jars will bear twenty-six pounds of sand and paper—all of which is shaken—and this is the appreciable movement of a tangible substance.

But a locust could make a jar of the proper pitch respond at a much greater distance than a man can. Hence the radius would be much more than seventy feet, and that last result I have given would be multiplied many times.

Does the reader say, "These results are preposterous?" Let me tell him something else that is preposterous. From my childhood I have heard it said that a little dog trotting across a heavy suspension bridge shakes it Dr. Hall himself mentions this fact:

"A dog trotting over a large bridge will shake violently the whole structure, provided his steps shall happen to correspond with the vibrational size and tension of the bridge.—*Microcosm*, Vol. III., p. 249.

I need not discuss the question of the timing of the foot-falls of the dog. The fact that this result can be brought about under any possible circumstances is enough for my argument. A dog, if he so desired, could trot back and forth half a day at a time, and thus cause the bridge to shake "violently" all the time. Now consider the weight of the bridge—hundreds of tons, and the weight of the dog—a few pounds, and the motion not minute, but "violent," the idea of the dog being able to bring about such a result is preposterous. Argument *à la* Dr. Hall.

A heavy weight on a suspension bridge causes it to yield very perceptibly. A light weight must cause it to yield an infinitesimal amount. But all physicists, as we have seen by a previous chapter, consider the amplitude of sound waves, in the vast majority of instances, to be infinitesimal. But more than this, the bridge can deflect only by the bending of very stiff wires, and the yielding of rusty joints. But the particles of air are exceedingly mobile (Dr. Hall will indorse this statement), and vibration in it occurs almost without friction.

Is it said that gravity explains this power of the dog over the bridge? I answer that it does not explain the horizontal vibration at all, and I do not believe it explains the vertical movement. It is easier to see how a locust can overcome the mere inertia of the air (neglecting the slight amount of friction) than it is to see how a little dog can overcome the inertia of so much iron, with the added resistance of stiff wires and rusty joints.

Dr. Hall comes forward as the champion of common sense in Natural Philosophy. That is a most excellent foundation; but the danger is that a man will rule out as contradicting common sense everything that is beyond his own observation and experience. It seems to contradict common sense that an animal covered with fur, and having many of the habits of the musk-rat, should have feet and bill like a duck. Yet I have seen the skin of such an animal.

So with physical forces. The books have for a long time given a certain law for hydrostatic pressure, which says that the pressure increases rapidly with the depth. The experience of divers confirms this law, for they can go but a very short distance below the surface when the pressure becomes unendurable. Stand-pipes, etc., though made of heavy iron, burst with the powerful pressure. Bottles weighted and let down into the ocean are either burst or have the stoppers forced in by this crushing power.

> "A brass vessel full of air, containing only a pint, and whose walls were one inch thick, has been known to be crushed in by this great pressure, when sunk to the bottom of the ocean."—*Olmsted's College Philosophy*, p. 120.

Whales, when struck with the harpoon, sometimes dive to enormous depths, as shown by the amount of rope they drag after them. If a whale of 3,600 square feet dives to a depth of 2,500 feet, the pressure upon it will be over 58,000,000 pounds, or more than the weight of 800 locomotives. That result certainly outrages common sense. Then, to follow Dr. Hall's example, we should reject the whole law of hydrostatic pressure. But Dr. Hall indorses this law. (See *Scientific Arena*, November, 1886, p. 89.)

The secret of Dr. Hall's mistake lies in his forgetting how easily motion takes place in a body when the forces that play upon it are equal. For months together I have worked with chemical balances that could be loaded with fifty grammes on either side, the beam, pans and index would weigh at least twenty-five grammes more, yet if the rider were changed so that there was a difference of one-tenth of a milligramme between the two sides, the whole mass was thrown into vibrations that would continue for perhaps two minutes. It turned with the 1–1,250,000th of its load. The makers guarantee these instruments, when sold for commercial purposes, to be at least that delicate. I have seen the statement that balances have been made that would turn with the 1–7,000,000th of their load. The air is in more delicate equipoise than we can hope to ever give any machine.

Suppose we have a tube of one inch sectional area, and one mile long. A plug which fits air-tight, but moves without friction, is pushed to the middle of the tube and the ends are stopped. Would it require much to move it the one-hundredth of an inch either way? "Yes," exclaims Dr. Hall, "there is a pressure of fifteen pounds to the square inch to be overcome." But this is just the amount of pressure it bears on the other side, so that it is in equilibrium. And to push the plug the one-hundredth of an inch either way would condense the air on one side, and rarify it on the other so little, that the amount of force necessary would be inconceivably small.

The favorite of all Dr. Hall's proofs is this "Locust Argument." In the full orchestra, of all the others whose attenuated corpuscles are to charm and enchant the world, none please the manager half so well as this violin soloist. This one musician, from the wilderness is to throw into utter confusion the "scientific parrots"—the enemies of God and men. But, above all, around this minstrel gather the mighty host of figures, almost innumerable, who, with their swords and their glittering spears, are to put to flight the armies of the alien. But this apparent discrepancy between the power of the agent and the work done has astonished others as well as Dr. Hall. Thus Prof. Youmans says:

> "It is marvelous how slight an impulse throws a vast amount of air into motion. We can easily hear the song of a bird 500 feet above us. For its melody to reach us, it must have filled with wave pulsations a sphere of air 1,000 feet in diameter, or set in motion eighteen tons of the atmosphere."—Quoted in Avery's *Handbook*, p. 100.

But, with the Doctor's permission, we will look somewhat carefully into this "Locust Argument." I care nothing for the absolute numbers he gives, but he values them highly—they are dearer to him than the apple of his eye; hence he should be careful to have them correct. But he is very careless.

A certain kind of locust, he says, can be heard a mile; consequently, four cubic miles are being "churned into condensations and rarefactions."* I have somewhere seen the statement, by some balloonist, that sounds do not rise very high into the atmosphere, that they are not audible to near the distance vertically that they are horizontally. In proof of this, he stated that the cheering of a crowd soon dies out, while the waving of handkerchiefs is still quite visible.

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* On page 130 the Doctor says: "It was a source of astonishment that so diminutive an insect—weighing less than a quarter of a pennyweight—could fill, as it did, four square miles, including, no doubt, a mile high, with its wonderful music!" He must mean *cubic* instead of square miles, and this is shown by the fact that he calls them so on page 134. This wonderful mathematician, who is to show that all others are frauds, does not know that there is any difference between a "square" and a "cubic" mile! That this is not a mere typographical error is shown by the fact that they are allowed to stand in the revised edition of 1878, which, he says, was "wholly rewritten."

It is well known that sound is not reflected with the accuracy that light is. When sound strikes a surface, it has a tendency to flow along that surface. The most of a sound strikes the earth obliquely, and, of course, yields more or less to this tendency. The sound that goes down is not lost, but spreads out and extends the borders. Were the locust high up in the air, the sphere through which it could be heard would, I suspect, be much less than a mile. Also, it is known that the loudness of a sound is determined by the density of the air where it is generated rather than the density where it is heard. But I can not say with certainty as to how far a sound audible for one mile along the surface could be heard vertically.

When, to his dismay, the ignorant scientist hears the hated locust a mile away, his friend the aëronaut, a mile above the enemy, hears nothing. The mass that is "churned" into "condensations" and "rarefactions" is not, I think, the half of a sphere of one mile radius, but the half of an oblate spheroid—a much less volume.*

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*Capt. R. Kelso Carter has this to say on calculating the volume through which the sound may be heard:

"A locust has often been heard at a distance of one mile. This is the first fact. Were the creature one mile from the earth, we could claim eight cubic miles as the volume of air within which the sound could be heard. But as the locust is always upon the earth, we are content with the plain fact that the sound is audible one mile in any horizontal direction, and one mile in altitude. This gives a half cube, or a prism of air containing four cubic miles. Of course the upper corners or edges of this prism are more than a mile from the center; but we presume that no one can question the small addition, if the air be still and the day quiet."—*Microcosm*, Vol. III., p. 236.

Then this adept in legerdemain arithmetic states his case. He says the drum-skin of the human ear has a superficial area of about one-fourth of a square inch, and weighs something over half a grain.

> "We have, then, only to suppose one tympanic membrane accurately and sensitively located in the space of each quarter inch throughout the four cubic miles filled by the sound of the locust, and as certain as there is any truth in the wave theory of sound, all these membranes must be thrown into vibratory motion, if stretched with the same tension as they are in human ears, because it is perfectly evident that an ear, if present at any quarter inch throughout this mass of air, would hear the sound of the stridulation, which, according to this theory, could only occur by the shaking of this drum-skin."—p. 177.

The Doctor wants us to believe that a locust could be heard a mile with the intervening space filled up so, and that because a drum-skin placed at any point within these four cubic miles would vibrate, that therefore, if it were all filled up with drum-skins, they would all vibrate at once! That is to say, if I can lift one brick I can lift a whole brick house at one time, for one brick is no heavier than any other! And this argument is put forth with an assurance which seems to say, "Sophistry well stuck to is as good as the truth."

The Doctor calculates that the drum-skins, when entirely filling the whole space in the manner described, would weigh 2,000,000,000 tons, the whole mass of which would be moved by the stridulations of the locust. It is undoubtedly true that an ear, when placed at any

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To calculate the volume of a hemisphere of one mile radius was seemingly too hard a problem for him, so he took the half of a cube and neglected the "corners!" And this from a "Professor of higher mathematics!" Capt. Carter saw that the problem involved the volume of a *hemisphere* of one mile radius. Dr. Hall himself becomes confused by the problem, and calculates the volume of a *sphere* of one mile radius. This, as he says, has a volume of four cubic miles. But Dr. Hall's problem involves but about half of it, and his mistake has multiplied all his results by two.

point within the space, would hear the sound; but what thinking man can believe that the sound would ever reach the drum-skins placed a mile away were the intervening space filled up so? It is doubtful to me if the sound would pass through 2,000 layers, yet these, placed as Dr. Hall places his—one-fourth of an inch apart—would carry us only about forty feet.

A rifle will carry, say a mile. The ball passes through every point along that space. It would pierce a half-inch board that might be placed anywhere along the line. Therefore, by Dr. Hall's method of argument, it is conclusively shown that a rifle ball will pierce a mile of boards set up in a compact row!

I have quoted Dr. Hall as saying that sand on a resonant jar would dance about when the proper note is sounded. I said that these jars might be placed on the surface of a hemisphere of seventy foot radius, and, allowing each membrane to weigh five grains, and each of these to bear three grains of sand, that the aggregate amount that would be in motion would be twenty-six pounds. Let us apply Dr. Hall's method of argument to the problem before us. A jar would be found to respond, if placed anywhere within the space; therefore the whole space may be filled up with these jars, and they will all respond at once.

A hemisphere of seventy foot radius contains about 660,000 cubic feet. Allowing 100 cubic inches to each jar, this space will hold 11,000,000 jars. The membranes and sand upon them will weigh about six tons; all of which is thrown into *visible* vibrations by sounding the proper note with the voice. I hope the Doctor does not consider this application of his method an infringement. As I have said if a mile is the limit of audibility for the sound of a locust, then only one layer of drum-skins can be counted, for this would quench the remnant of the sound. The surface of a hemisphere of one mile radius is about 22,000,000,000 square inches. Allowing a square inch of drum-skins to weigh two grains, this expanse will weigh about 3000 tons. The Doctor's mistake lifted this to 2,000,000,000 tons! This would be more excusable if it had arisen from a mere mistake in calculation; but it arose from a ludicrous misapprehension of what would be found the physical facts were it possible to subject it to experiment. Yet such is the intellectual tyranny that Dr. Hall exercises over his followers, that hundreds of men calling themselves educated have just gulped down his statement of this argument, and considered it mathematically accurate and unanswerable. This sway over the minds of his disciples is remarkable. He starts some new question in his magazine. Many opinions are expressed by different writers. At length Dr. Hall autocratically states his view, and after that there is but one opinion on the question among "substantialists," and that is Dr. Hall's view.

When a fundamental discovery (like the discovery of "induced currents" in electricity) has been made in the past it has been the signal of a host of discoveries founded upon it to be made. This has been a universal rule. Dr. Hall published what he declared a revolutionary discovery in physical science. Hundreds immediately accepted it. He expected to see his theory rapidly perfected and strengthened. Except in the refining away of the word "substance" his theory stands to-day exactly where it did ten years ago. Not a fact has been discovered whose existence was suggested by the new theory. Nobody could do anything to elaborate the theory.

This state of affairs might arise from either of two causes. A theory might, on almost any question, be brought forward which could be made, by its author, plausible up to a certain point, and beyond this point no one could carry it. Or the new disciples might be so utterly unfitted for the work, and so lacking in originality, that nothing could be expected of them. Both causes coöperated, I think, to give this stationary character to the corpuscular theory of sound.

Every leading scientific man, from Newton down to the present time, always found, even among his admirers, a large body of able men who opposed him on almost every leading doctrine he held. But Dr. Hall's sway over the minds of his adherents has been undisputed. The corpuscles of sound can not be so attenuated as the Doctor assumes them to be. I use the word "attenuation" in connection with the Doctor's theory of sound advisedly. I am only following his own example. The teaching of his book is that sound is a form of attenuated matter. I do not know that he has ever expressed a desire to retract anything in his book. On the contrary, he expresses so much satisfaction with his own work, and in such a way, that he leads one to think that he is confident that every leading doctrine in his book will stand the test of the most searching investigation. His book is full of the thought that sound is but a form of attenuated matter. He has however at times in his periodical writings, I think, tried to make this thought so intangible that it can not be discussed, but he has never retracted.

I give one extract from the "addenda" to his book, written, I suppose, after his theory had been all thought out and committed to writing, hence it expresses his mature opinion:

"*For if light and sound are substance*
Or attenuated matter,
As so clearly demonstrated
In the three preceding chapters,
Which can permeate the densest,
Most impervious of bodies,
Such as diamond, gold, and granite,
May not spirit be a substance
Far beyond the comprehension
Or the tangible conception
Of our gross material senses?

. . . . .

In this hurried exegesis,
Proving sound to be a substance
Real as is air or odor
I have aimed at broader questions
Than those merely contemplated
In their philosophic aspect,
Howsoever grand they may be
Viewed as scientific problems,
Questions which involve existence
Of a conscious, living ego,
When corporeal organisms
Join the elements of nature."—p. 334, edition of 1878.

As I have said, the corpuscles of sound in the Doctor's theory can not be so attenuated as he assumes them to be. We know this by the effects they produce if this theory be true. One fork will set another in unison with it in motion at a distance of fifty feet. And the tuning-fork is a heavy mass of steel weighing several ounces. Dr. Hall indorses this method of judging the magnitude of causes. He says:

"No physical effect can be produced without an adequate corporeal se."—p. 232.

The Doctor is right in insisting there must be some correspondence in magnitude between a cause and the effect. Particles of air are certainly attenuated, but with considerable velocity can produce decided corporeal effects—witness the ponderable bodies moved in a hurricane.

If the size of the moving body be lessened, the velocity must be correspondingly increased to maintain the momentum unchanged. A small rifle ball with high speed may have as much momentum as a large ball with low speed. In the chapter on the propagation of sound I have pointed out the distinction between the velocity of the *form* of the wave and the velocity of the *particles* of the wave. The velocity of the form of a sound wave we know. It is the velocity of sound, (about 1120 feet per second at 60° F.) The velocity of the particles that make up the wave we do not know. I see no reason for supposing that it is much greater than the velocity of the tuning-fork prong that gave it the push.

Dr. Hall says this is about eight inches per second. This is equivalent to the velocity of a pleasant breeze. It is not surprising that a tuning-fork can arouse sympathetic vibrations in a distant fork that is in unison with itself. For, (if an Ut^3 fork be used) 256 waves have dashed against its surface every second, and at times corresponding to its own time of vibration. We have all noticed that very slight pushes, if rightly timed, will give very vigorous oscillations to a person sitting in a swing. The eight inches per second is the *average* velocity of the tuning-fork's prong. Its velocity at the middle of the swing must be greater, and this, in strictness, is the push it gives the air. Thirty-five miles per hour is the velocity of a high wind. The air has mass enough even at this moderate velocity, less than that of a railroad train, to throw down trees. And it is not surprising that hundreds of waves of air of this velocity can set a bar of iron in motion when they come in times corresponding with its own vibrational number.

If it be said that sympathetic vibration can be explained by supposing that periodical pulses of sound corpuscles are thrown against the fork and set it in motion much in the same way, the reply is that the corpuscles must be inconceivably more attenuated than the particles of air, and their velocity is comparatively sluggish (1120 feet per second, they being merely projectiles), and to set in motion so heavy a body is too great a work for them to do, being small and having a slow velocity. Their momentum would be too small.

"No physical effect can be produced without an adequate corporeal cause." The fact that the sounding body is not dissipated shows that the corpuscles in the Doctor's theory are exceedingly attenuated. But there are just as powerful reasons for supposing them to have some considerable size. The Doctor (p. 161) explains how sound can pass behind some obstruction, by saying that the corpuscles sub-divide, secondary systems being thrown off from the first. These secondary systems are of less size, have less momentum, and hence the sound is not so loud. This sub-division must take place several times in intricate cases.

One tuning-fork will respond to another, even when a screen intervenes. The *parts* of corpuscles have mass enough to produce corporeal effect, though not so much as the primary ones.

As these minute parts of a corpuscle have this much mass, the original corpuscle must have had more; so much indeed that they cannot incessantly be taken from a body without perceptibly reducing its size. The corpuscles must be dividing incessantly and without reference to their surroundings. Some secondary ones pass behind an obstruction, but the obstruction did not make them separate from the primary. A wave is a unit, and the particles making up a wave have some connection one with another. Consequently when a wave strikes an obstruction, it can send a secondary wave behind the obstacle. But a volley of balls (either musket balls or sound corpuscles) is not a unit, but a collection of units having no connection between them (that is, neglecting universal gravitation), and when one strikes an obstruction it does not influence the others.

Suppose a tight board fence is one mile long. In it there is a row of holes three inches apart. A sound is passing along the fence, and a little passes through each hole. Dr. Hall says this is made possible by the division of the corpuscles. By the time the corpuscle reaches the last opening it must have divided over 20,000 times. Yet this minute fraction of a corpuscle is able to produce corporeal effects. But the original corpuscle must have been at least 20,000 times as large, and the primary corpuscle must have weighed as much as all its parts. And if an infinite number of such masses be taken from a body, it must, in the very nature of things, reduce its weight.

Dr. Hall discusses the nature and tenuity of odor and says there is an almost exact analogy between it and sound; as we have seen, the ancients also believed.

He says there is a kind of musk which, if placed room, will give it an appreciable perfume for a hun years, and yet undergo no loss of weight that ca detected. But I think every one must have noti that air has a saturating point beyond which it can go for those conditions. Place a quantity of perf in a room, close the door, enter again the next day note the amount of odor present. Try it again next day. It will be found to be about the same a the day before. This seems to mean only that particles rose until the air was saturated for those ditions, then the emissions ceased.

An analogous occurrence takes place in the ev ration of water. With a room of ordinary tightn there is a slow movement of the air into and out of room; hence a slow evaporation of the water to k the air saturated. Should the room be kept air-ti and the temperature constant, after the point of uration should once be reached evaporation would ce and the water would no longer lose in weight would remain in that condition indefinitely. Now that water in a strong breeze where the air, as fast takes up moisture, is carried away, and the water soon pass away in its emissions.

A grain of musk if placed in a close room keep it perfumed for years yet if set in a strong would probably pass away in twenty-four hours. Ev one knows that, when in the pocket, a handkerc will hold perfumery a long time (because the air ab it is confined) but when taken from the pocket spread upon the table, it is soon lost.

Dr. Hall is surprised that a grain of musk in a ti room can keep the air perfumed for years, and undergo no appreciable loss of weight. It seems to me it should do so for a thousand years just as well, for the musk does not lose anything after the saturating point be once reached, and this occurs after a short time. Every one knows that on opening a perfumery bottle the odor is not found to be stronger and stronger the longer the bottle may have remained closed. The amount, however, may be modified by some other conditions, one of which is temperature. The point of saturation is higher for warm air than cold, that is, it will absorb more. Cold reduces the number of particles of odor that may be given off, as though the texture had been rendered more firm. And, as far as I know, frozen bodies give off very little odor; though I do not suppose it is impossible for them to do so, as even ice will evaporate, as we see in the phenomenon of clothes "freezing dry."

As water emissions become more rapid the higher the temperature rises, and as an analogous case occurs with odor, so also we would expect that sounding bodies (on the corpuscular theory) would give off more sound the warmer they are. A piece of cold iron will ring loudly if struck, but if heated gives off almost no sound. To heat a bell destroys its resonant quality.

As most bodies become more elastic as they grow colder, this phenomenon is explainable if sound be vibrations which are transmitted to the air; but if sound be emissions, we would expect a body to be most resonant when softened by heat, the same as a piece of wax is most odorous when warmed.

It would seem that the story of words (sound corpuscles) becoming frozen in the air, and becoming audible again when thawed out in the spring, is no fancy sketch, but is within the bounds of possibility, indeed probability. To stop the waves of air is to annihilate them, but sound corpuscles can not be annihilated, for that, the Doctor thinks, involves the possibility of the annihilation of the soul! The Doctor also brings up the well-known facility with which a hound will follow a fox and says:

> "I now ask the puzzled reader who fails to see how the locust can fill an area two miles square with sonorous substance and not appreciably reduce its weight, to tell me approximately how much Reynard has reduced his feet in size and weight by the clouds of odor diffused along his track for a hundred miles."—p. 136.

The imagination can not conceive how great must be the tenuity of these emissions, yet they must have some mass which becomes reduced, or the track would not become "cold" in time. Most, if not all, of the odors given off by animals, are secretions that come to the surface of the skin. And these secretions are not in such slight quantities, but may amount to ounces or even pounds per day. The reduction in size of the fox's feet is really not involved in the case, no more than is the burning of the wick responsible for all the flame of the lamp. They only carry the odor to the surface as the wick carries the oil. The thousands of foot-falls doubtless wear away the feet of the fox, but these particles may add nothing to the odor that the hound detects. The particles of odor are undoubtedly inconceivably small, but I am fully pursuaded that they are not so near the infinitesimal as Dr. Hall would have us believe.

To Dr. Hall this "Locust Argument" is the dearest of all "the children of his brain." He rehearses it time and again. He insists that either the wave theory or his own must be true, that there is no middle ground, that nothing else is conceivable. The stridulations of his darling sets the Doctor's brain in a whirl every time he hears it, and he sings the song of victory over the hated undulations, and pronounces an eulogy on his beloved corpuscles. (By the way, I pronounce mine on the "lovers'" telephone. It is more romantic. Indeed, it is more instructive.) The ardor of most men would be cooled when they remembered that for every difficulty that existed in the old theory a hundred had arisen in the new. But so slight an obstruction in the way of his triumphal car does not trouble the gallant Doctor. The car of Juggernaut rolls on. From its gilded pinnacle floats a banner bearing the words "Down with the Evolutionists." The devotee waits no longer. The fact that there is absolutely no connection between the doctrine of evolution and the wave-theory of sound does not occur to the zealot. He remembers not that either might stand while the other fell into oblivion. The common-sense view of deciding each question strictly on its own merits arises not in his mind. He even thinks if it be proven that waves of air really break upon his ear that the same argument also proves that the fond hope that the waves of time break upon a farther shore is but a delirious dream. He hesitates no longer, but casts himself under the advancing wheels.

CHAPTER VI.

THE PHONOGRAPH.

Prof. Deschanel gives this explanation of the phonograph :

"Mr. Edison, of New York, has been successful in constructing an instrument which can reproduce articulate sounds spoken into it. The voice of the speaker is directed into a funnel, which converges the sonorous waves upon a diaphragm carrying a style. The vibrations of the diaghragm are impressed by means of this style upon a sheet of tin-foil, which is fixed on the outside of a cylinder to which a spiral motion is given as in the vibroscope. After this has been done, the cylinder with the tinfoil on it is shifted back to its original position, the style is brought into contact with the tin-foil as at first, and the cylinder is then turned as before. The indented record is thus passed beneath the style, and forces it and the attached diaphragm to execute movements resembling their original movements. The diaphragm accordingly emits sounds which are imitations of those previously spoken to it. Tunes sung into the funnel are thus reproduced with great fidelity, and sentences clearly spoken into it are reproduced with sufficient distinctness to be understood."—*Elementary Treatise on Natural Philosophy*, p. 939.

Dr. Hall says in regard to the telephone and phonograph :

"In the first place, in undertaking an explanation of these remarkable instruments, it is necessary to call attention to the obvious distinction, so frequently referred to in the preceding pages, between the incidental *air waves* sent off by a sound-producing body such as the vocal organs and the *sound pulses* themselves. These sound pulses, as I have tried to show, are generated by the molecular action, which occurs in the rapid vibratory motion of the sounding instrument (see p. 93), and are then propagated through the air by a law peculiar to sound, as electricity (another substantial emanation, not wave motion) is conveyed through the wire by an analogous law of conduction—not driven through it by the motion of the battery or dynamo machine, as sound waves are supposed to be driven through the air by the to and fro motions of the tuning-fork's prongs.

"Synchronously, however, with each vibration of the sounding body and each sound pulse thus generated, there is generated and sent off an *air wave* which is driven to a limited distance proportioned to the force of the vibrating instrument. These air waves, however, as I show, have nothing to do with the accompanying sound pulses, being only incidental to the production of sound, as the generation of heat in a dynamo machine is incidental to the generation of electricity, but no part of the electricity generated. Such air waves, therefore, must necessarily correspond in force and rapidity to the strength and rapidity of the vibrations, and consequently to the intensity and pitch of the sound. Hence, in talking in close proximity to the diaphragm of a phonograph, it is readily understood that these air waves, impinging upon the diaphragm, cause it to vibrate with a rapidity and force corresponding to the rapidity and force of the accompanying sound pulses, thus producing a record of indentations in the tin-foil; and when the needle of the phonograph is again passed over the line of indentations it causes the diaphragm to reproduce vibrations similar to those of the sounding body which originally produced the record. *Thus the very tones of the voice, as well as the words spoken, are reproduced.*"—p. 334.

In the edition of 1878 he says:

"By properly attaching a steel point to the center of a telephone membrane [he must mean phonograph membrane], so adjusted as to press into the delicate spiral groove of a revolving cylinder enwrapped with tinfoil, the vibration of the membrane, acted on by the waves accompanying spoken words, is made to record a corresponding variety of impressions on the foil in the form of delicate impressions of varying depths and undulatory lengths. Then, by re-revolving the cylinder in the same direction, with the point in the line of indentations, the membrane is, of course, forced through the same variety of movements which produced the record, and is thus made to regenerate the original words exactly, the same as when spoken."—p. 524.

In the previous parts of his book he has spoken contemptuously of these "incidental air waves" that accompany the sound pulses for a short distance. With a flood of language he has insisted that they are the most unimportant, insignificant (and, to use his own language), "meaningless" affairs. It is almost the gist of his book. He has poured forth his vials of wrath and contempt upon the devoted heads of acousticians for daring to think that these waves can be of any complex character, or can produce any intricate effects. After his book is published, his attention is called to the phonograph and telephone, and he adds an appendix in which he admits that air waves and the results accomplished by them may be as complex as was ever claimed.

Neglecting the hypothetical pulses of sound corpuscles, let us see what he admits concerning these air waves—"incidental" though he says they are. He admits:

1. That the vocal cords in action do throw off air waves.

2. That these waves of air move some short distance at least.

3. That the number of waves per second corresponds with the vibrational number of the sounding body.

4. That the amplitude of the air wave corresponds to, if it does not equal the amplitude of vibration in the sounding body, and changes with it.

5. That whatever complexity they may have had at the start they retain it to the last.

To see clearly that all these admissions are involved in his language please reread the above quotations with their contexts.

In examining the water waves thrown off when the stem of a tuning-fork is dipped into water, he found that the number is very small compared with the number of vibrations of the fork. There were perhaps five or six per second, and scientific men were ridiculed for saying that the sound waves in air could amount to hundreds or even thousands per second. He now admits that the "incidental air waves" cause the membrane to vibrate, and this, by its steel point, makes a "corresponding variety" of "impressions" on the foil. There must be a corresponding number of "impressions" and "air waves," too; for, if the cylinder be turned with the same speed as in the first case the pitch is unchanged.*

Now, please state why it is ridiculous nonsense to say that equally as many "sound waves" may be thrown off as these "incidental air waves,"—they are both waves in air. Can not one kind be thrown off as easily as the other, and as many of them? A tuning-fork making hundreds of vibrations per second makes just the same number of impressions on the tinfoil—then there must have been hundreds of air waves. The phonograph will grasp a sound of very high pitch—one of thousands of vibrations per second.

Dr. Hall thinks it impossible that waves of air should pass behind an obstruction and maintain their identity. He thinks they would be destroyed, or thrown into inextricable confusion, and at least they would be no longer intelligible. He says:

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* If the speed is greater, more depressions pass under the steel point and the pitch is higher.

> "Sound, however, acts in no such way, and, therefore, can not be the result of wave-motion. If the listener is screened behind an impenetrable wall, for example, or a building, the sound passes around it and enters the ear in perfect form, both as to pitch and quality, being only reduced in intensity; and if it consists simply of air waves, as the current theory teaches, then these air waves, unlike those of water, can lap around the building, enter the ear in an exactly opposite direction, and retain their perfect form and outline, though broken, distorted and stopped by the obstruction, *which is clearly an impossibility*."—p. 160.

The Doctor is always very emphatic on this point. A man can hold a screen before his face when speaking to the phonograph, and the words will be recorded, showing that the "incidental air waves," which the Doctor says operate the phonograph, passed around the obstruction without being thrown into confusion. If the "incidental" air waves can so change the direction in which they are moving, why can not "sound" air waves do as much?

Dr. Hall would doubtless explain the action of the "lovers'" or "mechanical" telephone by saying it is operated by the "incidental air waves." It will transmit words when any kind of a screen is placed between the speaker and the transmitting instrument. In some cases the waves must have gone a very tortuous course to get to the instrument. Yet the words given off at the receiving instrument have undergone a reduction in intensity only, they show no tendency toward confusion because of the tortuous course of the sound in air, showing that the waves have maintained their identity unchanged throughout all their windings. If the "incidental air waves" can thus follow a devious course without confusion, why can not "sound" waves do as much? They are both waves in air. If one is equal to the occasion, why should the other be so utterly helpless?

The belief has existed for some years among scientific men that these larger, primary waves bear upon themselves smaller, secondary waves, and these give rise to the "overtones" which give the characteristic quality to any sound. The idea that such complexity could exist in mere waves of air has been supremely ridiculous to the learned Doctor. The man, he thinks, who can entertain it must be lacking in the first principles of common sense. The overtones, he says, are caused by secondary systems of corpuscles thrown off by the first. But he admits (as I have just quoted) that the phonograph "reproduces the very tones of the voice." Hence the overtones are present which give rise to the characteristic quality. But the diaphragm caught the principal tone from the air waves (it makes no difference if they are only "incidental"), and, of course, it caught the secondary tones in the same way. For, we have seen that a secondary as well as a principal "air wave" has made its impression on the tinfoil. Therefore one system of waves has borne another—a condition of things that the Great Objector had declared impossible.

The "incidental air waves" have done exactly what he said they could not do, and what is his reply? In substance it is only this, Well, they can not produce these effects at any great distance. He had declared (p. 124) that the largest bell in the world could not produce these mechanical effects at a greater distance than twenty feet. After he penned that statement he learned that the telephone (which he says is also operated by the "incidental air waves") can be used with the speaker standing one hundred feet from the transmitting instrument. (Revised Ed. p. 525).

Consequently the "incidental air wave," bearing its secondary wave, ran that far and maintained its identity. But more than this; three or four persons may talk to the Phonograph at once, and it catches and repeats the words.

The Doctor thinks it astounding that any man can believe that the atmosphere can carry several systems of waves of sound at the same time. If the atmosphere can bear so many systems of "incidental" air waves why can it not carry as many systems of "sound" air waves?

If, after the primary waves with their secondary waves have run several feet, they can make their impression on the heavy iron plate of the phonograph compelling this to make its indentations on a piece of tinfoil which can in turn cause the diaphragm to vibrate, certainly there is nothing surprising in the fact that they can make their impression at a much greater distance on the infinitely more sensitive nature of the human ear, connected as it is with a marvelously delicate nervous system. Any clumsy piece of apparatus for analyzing sounds that man can devise will not compare in refinement of workmanship with that of the ear. The olfactory nerves can in a moment detect the presence of a subtance of which the most careful chemical analysis can find no trace.

The surprising thing is that any human device can catch and record the air-waves even at a short distance, not that the ear can take cognizance of them at a long distance. The "incidental air waves" have shown themselves fully able to shake the heavy iron plate, but let an ear be placed in the same position and, presto! the Doctor thinks it the very delirium of foolishness to suppose the ear could take any cognizance of the waves whatever!

But what are these "incidental air waves" really for? They have doubtless been vibrating for ages. But there was nothing for them to do. At last the phonograph was invented and they doubtless hailed the glorious day with exultant delight. At last they had found something which they could shake! But the phonograph has remained but a scientific curiosity without use (except to talk against the corpuscular hypothesis) and the "air-waves" doubtless felt that they were without a mission in the world. But the electrical telephone has of course been a great consolation to them. In this one thing, at least, they could help the commercial world. But the Doctor cruelly tells them we can do very well without their services. Because, forsooth, the telephone can be made to operate slightly with the diaphragm absent. Even when the diaphragms are present their amplitude of vibration is doubtless very small and we could not speak against the naked ends of the magnet without changing its position somewhat with regard to the surrounding wire.

It has long been known that if a current of electricity be sent around a bar of iron that this is shortened in consequence. It was also known that if the circuit be closed or opened a sound is given forth by the iron, called the "Page effect." It may be that the changing length of the iron bar, produces part of the sound, though most of it doubtless is of molecular origin. But I do not pretend to be able to explain the phenomenon in question, yet, both the facts I have mentioned look toward a vibrational rather than an emission theory of sound.

It is true many physicists think that molecular motion is involved in the action of the telephone, and it may be found to be involved in many phenomena of sound. And the Doctor catches this thought, "molecular motion"—I correct myself. He catches the *words* "molecular motion," and speaks of it as though it were only a modified form of his theory. Molecular vibration is no more closely related to his doctrine than mass vibration. Each alike requires the transference of motion and not of substance. To inject the conception "molecular motion" into the wave-theory is really to make but a very slight modification of the original idea.

Dr. Hall would doubtless explain the mechanical or "lovers'" telephone by saying it is operated by the "incidental air waves," though the electrical telephone would do as well in the experiments I am about to mention, and he says *it* is so operated. The Doctor says:

> "Whatever aërial movements are thus generated in the immediate vicinity of the locomotive *can not, by any possibility, travel at a velocity of more than four or five feet a second.*"—p. 118.

I put up a mechanical telephone with 100 yards of wire. A friend stood twenty feet from the receiving instrument and clapped two books together. The velocity of light is so enormous that we may consider that I saw the action the moment it occurred. But how long did it take the sound of the concussion to reach me through the instrument? If the "incidental air waves" moved at the rate of five feet per second it would take them four seconds to reach the transmitting instrument, and even if the sound passed through the wire instantaneously I should not have heard the sound until four seconds after I saw the movement. On the contrary the two impressions reached me at the same time, as near as the senses could judge. Consequently Dr. Hall is completely mistaken as to how fast "incidental air waves" can travel. A friend takes his place at the side of the phonograph, and I, thirty feet away, shout to his instrument; as the waves travel five feet per second he is not to turn the crank until six seconds afterwards, when the "incidental air waves" get there!

My feelings overcome me and I turn to another part of the subject. Having conceded the most essential points on the origin and complexity of sound waves, the Doctor next proceeds to yield the rest of the theory, namely their *reception*. It had been one of the essential points in his declaration of war that a membrane would respond to no sound except one having nearly its own time of vibration.* Now the diaphragm

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* The Doctor ignores some distinctions that physicists make. Thus Prof. C. K. Wead says:

"Sound vibrations may be *received* by bodies belonging to three classes, which merge into one another:

"1. By a body *incapable* of vibrating in the same period; the sound wave is then partly reflected and partly absorbed, the lost energy of motion appearing as heat.

"2. By a body *capable* of vibrating in the same period; part of the energy is absorbed by it, and in it *forced vibrations* are caused, as shown in the vibration of sounding boards, of plate of telephone and phonograph, of drumskin and fluids of the ear, and of the membranes of manometric flames, opeidoscope, etc.

"3. By a body vibrating *naturally* in the same period; part or all of the energy of vibration is then given up to the second body. and in it *sympathetic vibrations* are caused. Sympathetic vibrations are shown etc."—*Brief Lecture Notes on Souud and Light*, p. 9.

Prof. J. W. S. Arnold in speaking of the ear, says:

"If this membrane were simply free, if it were simply attached around an opening with nothing else in connection with it, we would hear constantly, when a fundamental tone was sounded, this ringing in our ears; but nature has provided a peculiar arrangement which holds good in physics, and which you see beautifully represented in the talking machine here, namely that it has damped, that it has checked this fundamental tone. About the center there is a little bone which is

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of the phonograph is merely a plate fastened at the edges. One does not have to try until he gets the proper pitch (namely, that of the diaphragm) before he can get the instrument to operate. On the contrary one can talk or sing with a varying pitch yet the diaphragm catches and responds to the sound.

If the diaphragm of the phonograph can do this why can not the drum-skin of the ear do as much? The sound of the phonograph has a nasal twang. Everyone knows that the sound of the telephone has the same disagreeable quality. Indeed, it may be true, and is, as far as my limited observation goes, that all vibrating diaphragms, operated as these are, have this quality. Dr. Hall has insisted, with great emphasis, upon the difficulty a diaphragm (bound all around, as it is) would have in taking those secondary vibrational forms that lead to the overtones.

I have somewhere seen the statement that all sounds from which the *even* upper partials are absent, have this nasal quality. May it not be that the restraint upon the diaphragm leads to the elimination of this set of vibrations and consequent harmonics?

Dr. Hall has admitted that what he calls "incidental air waves" may have marvelous complexity, travel

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attached to this membrane, and this is in connection with several others and acts as a damper or weight. Now, if we apply this to the central portion of the membrane, it will not then respond to the fundamental tone; but by *forced vibrations* other tones will be transmitted, and we will not have a confusion of the fundamental tone continually drummed in our ears. And this membrane instead of being flat is stretched backward, and this kills more than anything else, the fundamental tone. The result is that we have a perfect arrangement for transmitting sound."—*Scientific American Supplement*, April 13, 1878, p. 1894.

The membrane of the "artificial ear" used by the United States Signal Service is not allowed to be in unison with the sound to be measured, for it would then vibrate sympathetically, but the tension is such that it is in dissonance and hence moves only by forced vibrations.

intricate courses, and produce remarkable effects—in short, they can do exactly what the wave theory attributes to the sound air waves proper. What is this double set of workmen for when either is able to do the work alone? On the whole does not the Phonograph offer demonstrative evidence (as far as we can have demonstrative evidence on this kind of a question) in favor of the wave theory of sound? When the Doctor's attention was first called to the phonograph he, with his magnificent powers of claiming every thing in sight, declared that the instrument furnished confirmatory evidence in favor of his doctrine. But it is noticeable that in all his periodical writings he has seldom, if ever, called attention to this witness' testimony. I admire the Doctor's judgment. No "substantialist" should ever call attention to the Phonograph's "confirmatory evidence"; the witness is a "wave theorist."

MARCELLUS THOMPSON.

CHAPTER VII.

INTERFERENCE.

In discussing the phenomenon of Interference, Dr. Hall says:

> "As one evidence that the law is without foundation in science or in fact, we need no better proof than the test here distinctly prescribed by lecturer himself, namely, the placing of two unison instruments half a wave-length apart, and then sounding them with listeners stationed in line either way to determine by actual observation the truth or falsity of the principle enunciated. Professor Tyndall distinctly tells us that two such instruments would not be heard in line, however loudly they might sound or however distinctly one alone could be heard if the other was silenced."—p. 283.

Now I wish to say emphatically that Dr. Hall has completely misunderstood and seriously misrepresented Prof. Tyndall. I do not, however, mean to say that the misrepresentation is intentional. Prof. Tyndall, nor any other physicist, would say that if two tuning-forks be placed one-half a wave length apart and then each be bowed in the ordinary way that there will be any appreciable interference, if indeed any at all. Prof. Tyndall says this will occur if the *phases of vibration* are the same. And this condition puts an entirely different construction on the whole article from that which Dr. Hall has given. I can not do better than give the whole of Prof. Tyndall's article, and this I do, omitting only the illustrations.

"When two unisonant tuning-forks are sounded together, it is easy to see that the forks may so vibrate that the condensations of the one shall coincide with the condensations of the other, and the rarefactions of the one with the rarefactions of the other. If this be the case the two forks will assist each other. The condensations will, in fact, become more condensed, the rarefactions more rarefied; and as it is upon the difference of density between the condensations and rarefactions that loudness depends, the two vibrating forks thus supporting each other will produce a sound of greater intensity than that of either of them vibrating alone.

"It is, however, also easy to see that two forks may be so related to each other that one of them shall require a condensation at the place where the other requires a rarefaction; that the one fork shall urge the air-particles forward, while the other urges them backward. If the opposing forces be equal, particles so solicited will move neither backward nor forward, the aërial rest which thus corresponds to silence being the result. Thus, it is possible, by adding the sound of one fork to that of another to abolish the sounds of both. We have here a phenomenon which, above all others, characterizes wave-motion. It was this phenomenon, as manifested in optics, that led to the undulatory theory of light, the most cogent proof of that theory being based upon the fact that, by adding light to light, we may produce darkness, just as we can produce silence by adding sound to sound.

"During the vibration of a tuning-fork the distance between the two prongs is alternately increasing and diminishing. Let us call the motion that increases the distance the outward swing, and that which diminishes the distance the inward swing the of fork. *And let us suppose that our two forks* (*a* and *b*, Fig. 150) *reach the limits of their outward swing and their inward swing at the same moment.* In this case the *phases* of their motion, to use the technical term, are the same. For the sake of simplicity we will confine our attention to the right-hand prongs (*a* and *b*) of the forks, neglecting the other two prongs; and now let us ask what must be the distance between the prongs *a* and *b*, when the condensations and rarefactions of both, indicated respectively by the dark and light shading, coincide? A little reflection will make it clear that if the distance from *b* to *a* be equal to the length of a whole sonorous wave, coincidence between the two systems of waves must follow. The same would evidently occur were the distance between *a* and *b* two wave-lengths, three wave-lengths, four wave-lengths—in short, any number of whole wave-lengths. In all such cases we shall have coincidences of the two systems of waves, and consequently a reënforcement of the sound of the one fork by that of the other. Both the condensations and rarefactions between *a* and *c* [that is, off to one side of the forks] are, in this case, more pronounced than they would be if either of the forks were suppressed.

"But if the prong *b* be only half the length of a wave behind *a*, what must occur? Manifestly the rarefactions of one system of waves will then coincide with the condensations of the other system, the air to the right of *a* being reduced to quiescence. This is shown in Fig. 151, where the uniformity of the shading indicates an absence both of condensations and rarefactions. When *b* is two half wave-lengths behind *a*, the waves, as already explained, support each other; when they are three half wave-lengths apart they destroy each other. Or, expressed generally, we have augmentation or destruction according as the distance between the two prongs amounts to an even or an odd number of semi-undulations. Precisely the same is true of waves of light. If through any cause one system of etherial waves be an *even* number of semi-undulations behind another system, the two systems support each other when they coalesce, and we have more light. If the one system be any *odd* number of semi-undulations behind the other, they oppose each other, and a destruction of light is the result of their coalescence. The action here referred to, both as regards sound and light, is called interference."—*Lectures on Sound*, pp. 357-8-9.

Dr. Hall quotes part of this passage, but he begins at about the middle of the third paragraph, with the words, "now let us ask," etc. But any one can see that this method of quoting this passage is unfair; it leaves out the essential condition that Prof. Tyndall had laid down. The "substantialists" have just gloated over this quotation that Dr. Hall gives, and it does not seem to have occurred to one of them that it might be well to examine the original.

But does some one ask "How can we get the phases of vibration correctly arranged?" I reply that this is a somewhat difficult matter.

If Dr. Hall had examined the catalogues of physical apparatus he would have found that it may be accomplished in this way. The two forks are kept in vibration, each by an electro-magnet almost in contact with it, the current being under the control of a third fork of the same pitch. The corresponding prongs of the two forks may thus be made to move in the same or opposite directions.

Thus we see that this experiment in Interference requires costly apparatus. So let us study one that every person can try for himself.

If we take a small tuning-fork, such as singers use, set it in vibration and then slowly revolve it in front of the ear we will find there are four places where the intensity of the sound reaches a maximum and four places where the sound dies, nearly, if not entirely, out. By careful examination we find that the sound is loudest when the back of one of the prongs is directed toward the ear. Two more points of maximum strength occur, namely, when the sides of both prongs are placed parallel with the ear. The points of minimum intensity are opposite the four outside corners of the prongs. A person is sometimes uncertain as to whether the extinction of sound is total or not. Rubber is a poor conductor of sound. Let the fork be revolved *slowly* in front of a narrow rubber tube and the person listening at the other end will find that there are points where there is no appreciable sound.

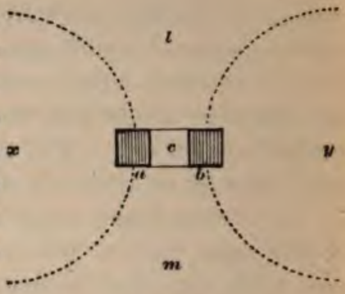

Fig. 3

Let us study for a minute, from Fig. 3, the action of the tuning-fork. The reader is supposed to be looking down upon the ends of the two prongs *a* and *b*. Suppose, at first, the two prongs are approaching each other. Then the air within the space *c* is being condensed, and a wave of condensation rolls at the same time into the quadrant *l* and into the quadrant *m*. Each wave tends to pass all around the fork. But the same movements of the prongs that condensed the air within the space *c* also produced waves of rarefaction within the quadrants *x* and *y*, and these move towards the fork, also tending to move all around the fork. But these waves of opposite kinds will meet, as we see, about along the dotted lines and neutralize each other, and this is where we find the sound is suppressed. The interference of a tuning-fork is so great that one prong will make more sound than both; this we know by passing a pasteboard tube over one prong, being careful not to let it touch the fork, when we find that the volume of sound is increased. Experiments on the Interference of the tuning-fork can be tried by every one, and without expense, and every person interested in these subjects should try them for himself.

If we take a large tuning-fork, such as those used by physicists, and turning the open end of the case toward a flat wall, there seem to be slight risings and fallings away of the sound as we walk softly toward the wall. I think this phenomenon is caused by interference. A wave, say, of condensation passes from the fork to the wall and is reflected, it meets a wave of condensation and intensifies it, it next meets a wave of rarefaction and tends to neutralize it, and thus we have a variation in the intensity of the sound.

A correspondent writes Dr. Hall as follows:

> "The lowest pedal-pipes of a church organ vary a great deal in effect in different parts of the building. So marked is this variation, that sometimes they seem to cease sounding. I have had a pedal note held and walked down the centre aisle, and at certain points the pipe would be inaudible, and a change of not more than one step would bring the sound with great power. Only last Sunday, while playing an organ in this vicinity, where the keyboard is twenty-six feet from the case, the D pipe in the pedal bourdon did not sound, as I thought. After church I remarked on the fact to the organist, who had been sitting only three feet from me, when, to my surprise, she pronounced it the best note in the pedal. I took the seat she had been occupying and found the note a splendid one. There was nothing between our seats and the front of the case, and the mouth of the pipe was in plain sight of us both. This fact you can verify by going into any church; for, in my experience of many years as an organist and tuner, I have never seen an organ that did not illustrate this point in a greater or les degree. And this is only in flue-pipes, or those constructed on the principle of the whistle. Reed-pipes, or those having their tones produced by a vibrator at the foot of the pipe, do not have this peculiarity, as far as my observation goes."—*Microcosm*, Vol. I., p. 218.

I believe that this phenomenon is caused by interference. The reflected waves from different parts of the church are so concentrated on some one point that they neutralize entirely the direct wave, and silence is the result. We would expect the phenomenon would be more marked in the pedal-notes with their long wave-lengths. Dr. Hall talks all around this subject for awhile, but never talks on it, and closes with these words:

> "Of course, this effect of apparent silence, described by Prof. Phelps, can not be attributed to the so-called law of interference, since that is caused entirely, as claimed, by the neutralizing effects of two systems of air-waves so traveling together that the condensations of one system will coalesce with the rarefactions of the other, and thus produce quiescence of the air—which, of course, is all that constitutes silence, according to the current theory. As the silence at certain points in the Boston church, as reported, is caused by a single pipe, and consequently by a single system of air-waves, interference is out of the question."—*Ibid*, p. 219.

This is a mere quibble on words, unworthy of a man who claims to be discussing broad principles. Dr. Hall knows as well as any one that a reflected wave is, as far as physical effects are concerned, a different wave and from a different source, as we see it illustrated in the echo.

On page 307 Dr. Hall begins the discussion of the experiment of dividing a stream of sound, letting the parts pass through two branches, one of which is half a wave-length longer than the other, and then reunite, when it is found that the sound is diminished in volume. This has been explained by physicists by saying that condensation has been made to fall upon rarefaction, and *vice versa*, and thus partial neutralization has taken place. Apparatus to accomplish this may be made in various ways. I made one for myself which is simpler and, I think, more convenient than the one Dr. Hall invented.

The Doctor says that the difference in length between the two arms is twenty-four inches (if an Ut3 fork be used) instead of twenty-six inches, as the wave theory requires. I said it was impossible, in finding the depth of greatest resonance, to determine the exact point at which we should begin to measure. So, likewise, it is impossible here. The diminution of sound begins at twenty-one or twenty-two inches, and continues through a space of six or seven inches, and I do not think any one can tell the exact point where it is most marked.

The Doctor also tried another experiment. He adjusted both branch tubes (both were of rubber tubing) to exactly equal lengths. He then found that by pinching one of the branches about twelve inches from the point nearest the fork where it separated from the other branch that this diminution of sound also took place. This, he thinks, completely proves that the instrument does not illustrate anything in interference. I believe I can explain, at least to a certain extent, the Doctor's experiment. I had a piece of brass tubing with a tubulure set at right angles at its middle point. I attached a piece of rubber tubing to the tubulure and longer pieces to the ends of the brass tube. I closed the outer extremity of this branch at the tubulure. I then allowed a stream of sound from the fork to pass through the straight tube. I found that pinching this branch tube at about thirteen or fourteen inches from the junction caused a diminution in the amount of sound that would emerge from the farther end of the straight tube. Pinching at any other point had no effect. The cause of this is easy to see. A wave, say, of condensation was passing along the longer tube, when it came to the junction part went up the branch, was reflected and got back just in time to fall in with a rarefaction and tend to neutralize it. For, it had passed over a quarter of a wave-length twice, and condensation and rarefaction are half a wave-length apart.

If a person examines Dr. Hall's account of his experiment he sees it must be identical in principle with this.

Here is another experiment in Interference. I took two large glass tubes of about two inch bore. I pushed corks into each until they had reached the depth of greatest resonance, which was at about thirteen inches. I then set them at right angles to each other, as in Fig. 4, with the open ends close to each other. A tuning-fork was then so placed that the back of one prong should be toward one tube, and the sides of both prongs toward the other tube. We see then that if the prongs approach each other and send a wave of condensation down the tube *b*, that the same movement will send a wave of rarefaction into the tube *a*, and that these two waves will neutralize each other on their return, and there will no resonance come to the ear, or very little. But if one jar be carefully taken away the resonance of the other is plainly heard. To my mind this is a very conclusive experiment, but it requires very careful manipulation to make it successful.

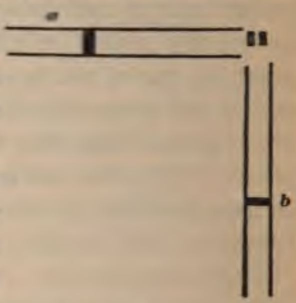

Fig. 4

If a circular plate of brass or glass be firmly fastened at its middle point, and a violin bow be steadily drawn across its edge, it will give forth a musical sound. If sand be scattered over the plate and the bowing be such that the fundamental sound is produced, the sand will be seen to arrange itself into two rows at right

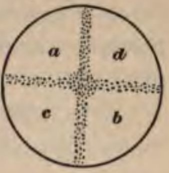

Fig. 5

angles to each other, as in Fig. 5. The sand moves from the parts of the plate that are in vibration to the parts that are at rest. As the sectors *a* and *b* move upward the sectors *c* and *d* move downward, and *vice versa*. That is, two adjacent sectors are always moving in opposite directions, while two opposite ones move in the same direction. Capt. R. Kelso Carter admits that this is the movement of a "Chladni" plate, as these are called. (*Microcosm*, Vol. III., p. 180.) Then when *a* and *b* have over them a wave of condensation, *c* and *d* have over them a wave of rarefaction, and *vice versa*.

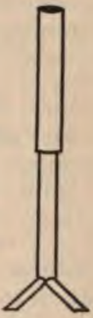

Fig. 6

Provide a tin tube about a yard long, made in two parts, so as to telescope one within the other. The extremity of one of the branches terminates in two smaller branches, as in Fig. 6. Place the sounding plate in a horizontal position, and hold the tube vertically over it, so that the two branches are over the sectors *a* and *b*. When the length of the tube is correctly adjusted, a marked resonance is heard; for these two sectors are moving together and are simultaneously throwing waves of condensation and waves of rarefaction into the tube. Now revolve the tube so that the branches shall stand over the two adjacent sectors, as *a* and *d*. These two sectors are moving in opposite directions, and as one sends a wave of condensation into the tube, the other sector sends in a wave of rarefaction, and, as we would expect, the resonance almost if not entirely disappears. The experiment can and we can see no connection between the various explanations. But the phenomena are evidently closely related, and the explanations should show some connection. Interference comes in as a general explanation, and simplifies the whole subject wonderfully.

CHAPTER VIII.

THE DECREASE OF SOUND INTENSITY.

On page 161 the Doctor begins a web of sophistry, and prolongs it to a great length—indeed, until he gets lost in his own labyrinth, and then, thinking every body else to be confused like himself, he characteristically exclaims, "The whole world is bewildered."

The gist of the question is this: Does it make any difference what shall be the unit of measure in stating ratios? The Doctor says it does, and argues in favor of his view most strenuously. According to him, synonymous expressions will not do, and involve an absurdity. If the body to be measured is large and we assume a small unit of measurement, it does not become a mere question of inconvenience. It involves a gross and inexcusable error in the result. Or it is the same unpardonable sin to assume a large standard when the object to be measured is small. Concerning Prof. Tyndall, he says:

"I say accidental, because it is entirely certain, in reading his statement of this law governing such ratio of decrease in loudness 'as

> the square of the distance,' already quoted, that he had not the most remote idea that it would make any difference what measure was employed in computing such comparative decrease—supposing, as any one can see by reading his statement, that the result would be exactly the same whether we used miles, rods, feet or inches, or otherwise, he would surely never have employed 'feet' without some sort of qualification as to the range of the sound to be taken into account, thus commiting himself, as he has done, to a fallacy in science of which he will be ashamed as long as he lives."—p. 165.

Prof. Tyndall undoubtedly thought it makes no difference what shall be the standard unit of measure in estimating the decrease of sound. And a man need only have three things to lead him to agree with Prof. Tyndall, namely, a clear head, a moderate amount of arithmetic, and good common sense.

The computation of the decrease of sound is always a ratio, the comparative strength of the sound at two different points in its journey; one of these points, however, may be the origin. The Doctor says:

> "But the clearest demonstration of the superficiality of Prof. Tyndall's use of 'feet' in his ratio for determining the decrease in a sound (leaving us to infer that the same measure was applicable to all sounds), is the fact that the entire range of many sounds is less than a foot."—p. 165.

Again he says:

> "Another sound of greater range or greater intensity at the start, might have its proportionate decrease in intensity approximately computed by employing 'feet' as the measure, while a very loud sound, such as that of the steam siren, having a range of ten miles, would evidently require a long measure to even approximate the true proportion."—p. 164.

The Doctor and I take a walk together. We come to two tall buildings, and one of us says, "I wonder what their relative height is?" We ascend the lower building; I let down a string and say, "This one is 600 inches high." The Doctor rends his garments, casts ashes upon his head, and exclaims: "What! You certainly do not intend to use so short a unit when the distance to be measured is so great? Do you not know that in calculating ratios the unit of measure must be a long one if the distances to be compared are long? That is the basis of a large share of my philosophy." I reply: "We have begun with this unit, let us finish it so. Possibly you can get some data for another article in your paper to show what fools the physicists are."

I measure the other building and say, "This one is 720 inches high." That is, their two heights are as five to six. If one-fifth of 600 be added to itself it will equal 720. "Now," exclaims the Doctor, "take a reasonable standard—a foot, for example." I take the measure, and reply, "The height of one building is fifty feet, of the other sixty. The ratio between these two numbers is the same as before, that is, as five to six." The Doctor says, "Well, that is strange," and we continue our walk.

We meet an acquaintance, a little boy who has just killed an enormous snake with a pole one rod long that some surveyors had been using. I say, "My boy, the enemy you have slain must be about twice as long as yourself. I will use this pole to calculate the ratio." The portly figure of the Doctor bounds into the air as he exclaims, "Now I have you. Your unit of measure is greater than the distances to be compared, and I know that will not work." I measure, and find that the boy is one-fourth of a rod, the snake is one-half of a rod; the ratio between them is as one to two. I say, "I see this rod has the feet marked on it. I will use a smaller unit." I find that the boy is four feet one and one-half inches, the snake is eight feet three inches. The ratio between these is as one to two, the same as before.

The Doctor says: "I lost faith in the books on Natural Philosophy a good while ago, and now I have lost faith in all our books on arithmhtic. I see this is another enemy of substantialism that must be gotten out of the way."

The Doctor seems depressed in spirits, and we return to our homes, he to write an editorial on the stubborn resistance of the "scientific parrots" to truth, and I to continue my reading on the "Law of Decrease of Sound Intensity," as presented by Dr. Hall. My eyes fall listlessly on the following words:

> "Why did not this careful physicist, if he is as careful as he is reputed to be, adopt meters, or rods, or inches, or furlongs, or miles, or leagues as his measure, instead of 'feet'? Possibly we shall find out the reason after a little. Had he employed rods, for example, as his measure for determining this decrease in loudness as the square of the distance from the sounding body in the place of feet, we would find the sound of the steam siren, at a distance of ten miles, diminished in loudness only the one 10,000,000th, instead of the one 2,000,000,000th, as recently seen to be the case when 'feet' were employed as the measure," etc.—p. 163.

Though this problem is not deep, the Doctor seems to have gotten beyond his depth in this calculation and misunderstood his own figures.

1st. Taking a rod as the unit. The intensity at ten miles (3,200 rods) is found by this law to be the one 10,000,000th of what it is at one rod from the source of sound.

2nd. Taking a foot as the unit. The intensity at ten miles (52,800 feet) is found to be the one 2,000,000,000th of what it is at one foot from the origin.

That is the secret of Dr. Hall's difficulty. When a man insists on using two units of measure in the same problem he can come to astonishing results—that is, astonishing to himself but to no one else.

Yet passages like the above quotation the Doctor's admirers call "using figures with crushing power."

In the arithmetics there are given directions something like these for stating a problem in proportion:

"Write the third term. If by inspection of the problem it is seen that the desired answer should be greater than the third term, write the larger of the remaining numbers for the second term and the less for the first."

So in applying a rule in Philosophy, is one not allowed to use his common sense? Dr. Hall even appears as the champion of common sense in science, but he wants us to abandon our own and use his. But must we all sneeze when he takes snuff?

The best rules are useless to him who has no idea of propriety in their application.

I once heard of a father who was much troubled by his son's hasty and inappropriate remarks. He talked to the boy very seriously about it and told him to think three times before speaking.

A few days afterward the father was standing with his back to the fire when the boy said, "Father, I think." "That is right, my son," replied the father, "I am glad you have not forgotten what I said."

"Father, I think," said the boy. "You are learning rapidly," replied the father. "Father, I think." "That is right, my boy, now what do you think?"

"I think your coat tail is on fire." A hasty examination that was then made by the father showed that the boy had thought correctly.

The rule was good enough, but it was worse than useless to him who had no sense of fitness as to its application.

So this law for the decrease of sound has been useless to Dr. Hall. If the Doctor wishes to reverse the direction let him also reverse the rule. Instead of saying that the intensity of a sound varies inversely as the square of the distance from the center, let him say the intensity varies directly as the square of the distance from the circumference.

At four miles from the siren the sound has one-fourth the intensity that it has at two miles, and at eight miles it has one-fourth the intensity that it has at four miles.

Or, if he wishes to calculate from the outside, let him place the numbers directly, instead of inversely, and he will find that the sound is four times as loud at four miles from the circumference that it is at two miles from it.

But, after all this tirade about "the infinite nonsense of the whole formula," it appears that the Doctor believes in the truthfulness of the law himself, for at the bottom of page 155, and several times afterward, he assumes it in stating his own doctrine as to the decrease of sound.

And the reason he assumes it is this. He says the intensity of a sound will depend on the number of corpuscles that strike the ear. The corpuscles are passing out in a spherical shell. They pass to a larger shell, and can not be so thick, for they are spread over more space. But when the radii of spheres vary, their surfaces vary according to the mathematical law we are discussing.

The law has forever been given as a mathematical law, not experimental. And the Doctor found himself driven by mathematics to use it. I will speak of its experimental side again.

Just for one moment look at the following. It is a triumph in the art of muddling one's self:

> "Now, this sound [of the midge], like all others, decreases in loudness according to the same uniform law, call it as the square of the distance from its source, if you like, to the extreme limit of its audibility, which it does as literally and truly as does the sound of a steam siren with its effective range of ten miles. Yet, how laughably absurd it would be to apply Prof. Tyndall's measure of 'feet' to the music of these ephemera. Let us try it. If the distance from the midge be two feet, the loudness of the sound will be one-fourth; if the distance be three feet, the loudness will be one-ninth; if the distance be four feet, the loudness will be one-sixteenth, and so on. Yet the sound entirely ceases within a single foot, and thus passes through all the gradations of decrease as the square of the distance, and even through a greater progression of diminution within this foot than the sound of a fog-horn passes through in a range of ten miles, since it is still distinctly heard at that distance. To employ 'feet,' therefore, in computing the ratio of decrease in the loudness of the sound of a gnat would be a measure about as much out of proportion one way as it would be enormously too small when applied to the sound of a steam siren."—pp. 165–6.

Now, is it absolutely incorrect to state the height of a man in miles, *i. e.*, in a given fraction of a mile?

And, if the height of two men be so expressed, can not the ratio be made out as well as though their heights had been expressed in feet and inches?

Will not the ratio in the two cases be the same?

Does it make any material difference, or is it not a mere matter of convenience?

Can not the weight of the Doctor's favorite "Locust" be expressed in tons as well as in grains, or has the "Singing Evangelist" of the new dispensation in Philosophy so dissipated himself in "sonorous corpuscles" by this time that the fraction is beyond the power of human endeavor to express?

Truly, Doctor, it makes no material difference what unit be taken in applying the law that sound decreases inversely as the square of the distance,—it may be miles, rods, feet, inches or any other standard; however, the same standard must be maintained throughout the same problem.

And when in two problems two standards have been used, their results can not be compared without first reducing them to the same denomination.

Is a man sixty inches in height taller than a man six feet simply because the absolute numbers seem to indicate it?

It might seem wholly unnecessary to remind a Doctor of Laws of such elementary principles, but the case plainly demands it.

But the truth is the Doctor's intellect, as I have said, had previously become demoralized in trying to apply the rule backwards, which he accomplished in the following amazing manner:

> "There is no escape from this, for we can almost use the Professor's own words, and say. At two feet from this ten-mile limit, passing toward the siren, the sound is four times as loud; at three feet, it is nine times as loud; at four feet, it is sixteen times as loud; at ten feet, it is 100 times as loud; at 100 feet, it is 10,000 times as loud; and at 1,000 feet, it is, of course, 1,000,000 times as loud.
>
> Yet 1,000 feet nearer the siren, at such a remote station (less than the fiftieth of the distance), would evidently not make a difference in the loudness of the sound, which could hardly be detected by the

most sensitive ear, though Professor Tyndall's highly (!) scientific formula makes the sound increase to one million times the intensity in this comparatively trifling space.

Can a theory be worthy of this enlightened age, or make any claim upon the intelligence of the reader as a scientific hypothesis, which depends for its existence on the inculcation of such a monstrous fallacy of science as this ratio of decrease in sound, gravely formulated by this eminent physicist?"—p. 153.

As I have said, the law is generally stated in words, something like these: "The intensity of a sound varies inversely as the square of the distance from the sounding body."

The manner of applying it may be illustrated in the following problem:

What is the relative intensity of a sound at two and four miles from the sounding body?

The square of two is four, and the square of four is sixteen, and the intensities will be to each other, inversely, as the squares, that is, as sixteen is to four, or the intensity is four times as great at the nearer station.

Or, if the Doctor wishes to do so, he can first reduce the miles to inches, and then make the calculation, and he will be surprised to find that the ratio comes out the same that it did in the first case.

And, as we have just seen, the rule is like a locomotive—works as well backwards as forwards—if we only know how to reverse the valves.

Many physicists have pointed out the fact that there must be some loss of energy by friction as the sound passes through the air, hence the decrease of intensity should be greater than the law requires.

To my mind many physicists have failed to note a point thus discussed by Prof. Avery. Quoting from a scientific friend who is pointing out the difference between the loudness and intensity of sound, he says:

"There exists the same distinction between quantity and intensity as in electricity. Attached to the violin, the whole of the wood vibrates, though with no greater amplitude than the string, or even less, but the quantity of tone is far greater.

"So Tyndall's deal rod, reaching down to the sounding-board, five stories below, had as much amplitude of vibration, and as much intensity of sound, as when a violin was laid on its upper end, but the quantity (and loudness) was, in the former case, small and in the latter large.

"So a battery of large cells is related to a battery of small cells.

"An intensity battery of ten cells gives more power to an electro-magnet than a single cell does, notwithstanding magnetic power is said to depend on quantity.

"In fact, it depends on both quantity and intensity.

"So loudness is the result of both intensity and quantity in the vibrations of the air or other medium of sound. I have a glass bell which yields to the bow a painfully intense sound which is yet not at all loud. Some bass viols yield a great quantity of sound (like some human voices) which, after all, penetrates but a short distance and sound 'hollow.'"—*Handbook*, p. 101.

Prof. Henry says:

"There is a difference between the quantity of sound and the loudness. Two sounds may be equally loud, when heard near by, yet differ very much in regard to their being heard at a distance, the loudness depending upon the intensity of sound or on the amplitude of vibration of the sounding body, while the quantity of sound depends on the extent of the vibrating surface."*—*Scientific Writings of Joseph Henry*, Vol. I., p. 490.

And Prof. Rossiter, from whom I have quoted before, says:

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* I have read that the sound of the cannonading in battle is often heard to a remarkable distance. But certainly to the ear a peal of thunder is louder than the report of a cannon. But every one knows a storm need not be very far away for the thunder to be inaudible. The thunder is the louder but the cannon can be heard the farther. I have never seen a discussion of this, but the distinction pointed out in the text may be found to throw some light on it. Lightning is not supposed to add any gases to the air, yet we all know that a loud roar of thunder will shake a house.

"So that we have to consider three things in estimating the intensity of a sound, *i. e.*, the force with which it falls upon the ear.

"(1) The amplitude of a sound-wave, *i. e.*, the distance each particle moves.

"(2) The velocity with which the particles move.

"(3) The distance which the sound has traveled.

"This last is quite distinct from the motion of each particle, and depends entirely upon the distance between the origin of sound and the ear. Thus, if I put a watch first close to my ear, and then on a table at the other end of the room, the distances are very different, but the amplitude of the sound-wave, and the velocity of each particle of air, in moving to and fro, are the same, *i. e.*, the watch does the same work in either case.

"Practically, the intensity of a sound is the force with which the ear is struck by the particles of air in contact with it.

"If the ear were at the limit of the first wave (1), it would be struck with greater force than if at the limit of the second wave, and generally the greater the distance the less the sound, because of its diffusion. The law of diffusion for sound, as for light and heat, is inversely as the square of the distance, *i. e.*, at twice any given distance a sound is reduced to one-fourth, at three times that distance to one-ninth."—*Elementary Handbook of Physics*, p. 22.

We notice that Prof. Rossiter states the law with an indefinite standard—"any given distance."

Prof. Tyndall used a definite standard (foot), and Dr. Hall, in his attempt to twist that fact into an argument, rendered himself supremely ridiculous. Dr. Hall is astonished that so many of the ablest men are satisfied with a rapid examination of his book and refuse to read it.

The Doctor's explanation, that it is bigotry and intolerance, does not meet the case.

The real reason is this. Sophistries, like these arguments of his on the decrease of sound intensity, are seen in a moment by any intelligent man who is accustomed to study physical problems, and he thinks it a waste of time to wade through all that mass of wordy trash.

As Prof. Rossiter says, this law of inverse squares is also given in the philosophies as applying to light.

The electrical light companies have for years been using this law to calculate the illuminating power of their lamps—stating it as so many "candle power." The same line of reasoning led to its being formulated for one as for the other.

When these companies have so many hundreds of thousands of dollars involved, is it not strange that they have never discovered that the law they have been using so long is ridiculous nonsense?

Theoretical errors become quite apparent when looked at through a piece of coin.

Every one knows that the velocity of water is greatly reduced by flowing through small pipes, and that the retardation is greater the smaller the tube.

This has always been attributed to friction—the simplest and most satisfactory explanation.

But it is known that the velocity of sound in tubes is not so great as in free air, and that the retardation is greater the smaller the tube.

Discussions of this are not difficult to find. I give one quotation:

> "M. Kundt has established that the velocity of the propagation of sound diminishes with the diameter of the tube,—the diminution, however, only becomes sensible at a diameter equal to one-fourth of the length of the wave of sound or note yielded. Above this diameter, and in larger tubes, the velocity remains the same.
>
> "He finds, also, that the diminution of the velocity of sound in tubes full of air increases with the length of the waves of sound."—*Kneeland's Annual of Scientific Discovery for 1869*, p. 178.

Is not this most easily explained by saying that the waves of air meet with resistance by friction against the side of the tube?

A wave is a unite, and hence can be retarded.

But a volley of pellets is not a unit, but a collection of units, and all would not be impeded by a resistance that the ones at the side might meet with.

A cannon ball fired through a tunnel would have as great a velocity as in the open air.

Now, there is a consideration that applies to the whole of the preceding discussion, and we will spend a few moments upon it, though it is a subject much too large to be discussed in an article of such humble aspirations as this one, even if I had the ability to treat it profoundly, and which I have not.

The Doctor often speaks of one sound being so many times louder than another, as judged by the ear.

Scientific men have often called attention to the fact that we can not say with certainty that one sensation is so many times stronger than another.

We can state with some certainty that two sensations of the same kind are about equal, but we can not state ratios between unequal sensations with any degree of exactness.

This fact is recognized in commercial transactions. One sugar is said to be two and one-half times as sweet as another. That ratio was not determined by first tasting one piece and then the other.

One way of deducing such a ratio is this. A given quantity of water, say a pint, is sweetened with perhaps an ounce of sugar. Small quantities of the sugar with which the first is to be compared are added to a second until the taste judges the second pint to be as sweet as as the first. If two and one-half ounces were required to make the sweetness of the second equal to that of the first, the first sugar is said to be two and one-half times as sweet as the second.

The tongue can decide that one sensation is stronger than another, but it can determine no ratio except equality, and not even that with exactness. So with all the sensations.

If first one lamp be brought into a room and then four more, does it seem five times as light in the second case as in the first?

It seems lighter, it is true, but does it appear five times as light?

It seems to me that every one must answer in the negative, yet five lamps certainly make five times as much physical disturbance as one lamp. It makes no difference to the argument whether that disturbance be waves of ether or pulses of corpuscles. Thus we see if a sensation be once aroused, to increase greatly the exciting cause does not increase the sensation much.

Sound has both its objective and subjective side. Webster gives these two definitions of sound:

> "1st. The perceived object occasioned by the impulse or vibration of a material substance affecting the ear; a sensation or perception of the mind received through the ear, and produced by the impulse or vibration of the air or other medium with which the e r is in contact; the effect of an impression made on the organs of hearing by an impulse or vibration of the air, caused by a collision of bodies or by other means.
>
> "2nd. The occasion of sound; the impulse or vibration which would occasion a sound to a percipient if present with unimpaired organs."

This question has often been asked. A tree falls on an uninhabited island, where there is absolutely no animal life, is there any sound caused by the fall?

This question is susceptible of two diametrically opposite answers, according to which definition of the word sound we take.

Both barrels of a shot gun, when fired together, make more noise than either fired alone, but I do not think that any one who notices carefully will think that the sound is really twice as loud. Yet the physical disturbance must be twice as great.

A "double quartette" does not appear to sing twice as loud as a "quartette."

I have heard a solo singer fill a large hall with sound and have wondered whether several such singers would not make a sound that would be painful in its loudness. Yet we know that a chorus of several hundred voices has a tone of only pleasant fullness.

A single musket makes a noise that is almost stuning in its intensity; the muskets of a regiment do not paralize the auditory nerve.

These facts are so strongly marked that scientists have divided the subjects into two departments:

(1) The subjective, physiological side; sound regarded as a sensation.

(2) The physical, or mathematical side; sound regarded as a physical disturbance.

This second division is the one considered in the text-books.

The first is almost ignored, and for the very best of reasons—because almost nothing is known about it.

The writers think they are not called upon to expatiate at great length upon a subject they know so little. Dr. Hall did not know which side of the subject he was treating, and many of his followers do not know which side of the subject they are considering, yet some of these say they have been teachers of physiscience for years.

So difficult is the sensational side of acoustics considered to be, that physicists have almost universally considered the question purely on the mathematical side.

They have known well enough that if the physical disturbance which we call sound be increased, that the sensation produced by it will not appear to be increased in the same ratio, but they have generally depended on the mathematical calculation and have not tried to measure the sensation.

I tried a rude experiment which may throw some little light on the question why the intensity of a sound does not seem to be in proportion to the physical disturbance causing the sensation.

I stretched a cat-gut string three feet long in a horizontal position.

I hung and ounce weight upon its middle point. It sank the .5 of an inch. I then hung an ounce weight from that. It sank the .3 of an inch. I then attached another ounce. It sank the .2 of an inch.

It may be that a slight push can cause the tympanum to yield considerably, while a hard push can not cause it to deflect much more.

And the sensation may be in proportion, not to the energy, but to the amount the tympanum yields.

But this is only a wild guess and is, I know, unsatisfactory.

CHAPTER IX.

WAVE MOTION.

Waves are generally divided into two classes: 1. "Gravity waves," and 2. "Waves of elasticity." The nature of the force that keeps up the vibratory motion is instantly seen to be the basis of the classification. In one it is the gravity, in the other the elasticity of the medium. Water-waves belong to the class of "gravity waves;" sound-waves belong to the other division—"waves of elasticity." In each the motion of a particle is closely allied to that of a pendulum, a swinging of the particle, with a varying velocity, back and forth past its point of equilibrium, but at which point it will finally come to rest.

"Gravity waves" may be divided into three classes, but which merge into each other: 1. "Stationary waves." 2. "Progressive waves." 3. "Waves of translation." "Stationary waves" are not common. A trough of water can be made to produce them by being shaken skillfully. In them the particle moves in a verticle line. The *form* of the wave even does not

appear to advance. It does not make the impress
on the eye that there is any progressive motion.
crest merely appears to sink until it becomes a trou
and then rises again in the same place. They can
seen rather frequently among wharfs after the water
been rough.

"Progressive waves" are by far the most comr
of all. These leave the impression on the eye that
wave is rapidly moving forward. But if the wave
studied, it is found that it is *form* only that has
progressive motion, not the material of the wave. P
Rossiter, as quoted in the first chapter, brings
thought out clearly. The motion of the particle
the wave, if studied by foreign particles distributed
it, is found to be up and down almost in a verticle li
There is a very slight movement of the particles
ward, and especially is this true if the water
too shallow, but the excess of water goes back ir
"under-tow." If the wave comes into water too s
low for waves of that size, the base of the wave is
tarded—it is said to "drag bottom"—while the to
the wave maintains its velocity, and at last falls forw
forming "breakers." "White-caps" are formed by
wind blowing off the top of the waves.

In the "wave of translation," caused largely
long wave "dragging bottom," not only the forr
the wave moves forward, but the particles of the w
also have an onward movement. Hence a wave of
kind may be very destructive. The best exampl
the "Great sea wave," caused by earthquakes. In
the destruction of Lisbon this wave of translation
as destructive to life as the earthquake itself.

Thus far I have spoken in general terms, and w out regard to the minutia of scientific accuracy. But it is of the utmost importance to note the following distinction. In all these waves (except the last) the particles making up the wave move in almost a verticle line, because gravity acts in a verticle line. But when we come to "waves of elasticity" we can not say that the particles move in a verticle or horizontal line, or any particular direction as related to these.

A water-wave is always propagated in a horizontal direction because the surface of a liquid is horizontal; the particles of which the wave is made up, however, move in a verticle line, because gravity so acts; hence the particle always moves *across* the "line of propagation." But a wave of elasticity (as a sound-wave) may be propagated in any direction—as up, down or horizontally.

In the sound-wave the particle does not vibrate across the "line of propagation," but *along* it. They are "longitudinal," not "transverse," vibrations.

Water-waves have "crests" and "troughs." Waves of elasticity have a point of "condensation" and a point of "rarefaction," which are generally taken as corresponding to the "crest" and "trough" respectively. Thus we see that while these two classes of waves are closely allied, yet they have a distinct line of demarkation between them. And, unless the difference, as well as the similarity is understood, it is impossible to understand the current doctrine as to the propagation of sound.

Dr. Hall has been at great pains to explain to the world that he does not understand it, but that would have been very evident even if he had not said half so much about it.

In sound-waves we see that the particle moves back and forth along the "line of propagation" in which the sound is moving—hence tangential rebound occurs the more easily. In water-waves the particle moves across the line of propagation, that is, more or less parallel with the obstruction. In sound-waves the particle vibrating, as it does, along the line of propagation, strikes the obstruction a real blow.

On the subject of tangential rebound Dr. Hall says:

> "Tangential rebound, which is all there is of reflection, is only predicable, therefore, of the atoms of a substance moving forward with a certain velocity, being suddenly impeded by a resisting surface, as a child can fully comprehend in bounding its ball."—p. 122.

The Doctor "builded better than he knew" that time.

I have said that in water-waves the "line of propagation" is always horizontal, and as the particles of water vibrate across this line they must move *up and down.* But the "line of propagation" in sound-waves may be horizontal, vertical or any other direction, and as the particle moves *back and forth* along it we can not apply the phrase "up and down" to them, but we can say they move "to and fro," referring to the line of propagation. Any one can instantly see that a person who fails to grasp this thought has utterly failed to understand wave-motion. This distinction is at the gist of the whole matter.

Yet, after all that Dr. Hall has written on the subject, he does not understand the distinction; in fact, he does not seem to know that such a distinction exists. For one proof only, but a conclusive one, of this statement, please read this extract:

> "All writers on sound tell us that the material particles of any body constituting the sonorous wave, though they do not travel forward with the undulation or swell, yet have a 'to and fro' movement, *once up and once down* as each wave passes, as observed in the up and down movement of a chip floating on the surface of water disturbed by waves."—p. 236.

This passage overwhelmingly proves that Dr. Hall thinks the phrase "up and down," by which physicists describe the motion of water-waves, or "waves of gravity," can also be used to describe the motion of sound-waves, or "waves of elasticity," a motion that physicists describe by the phrase "to and fro." He thinks those phrases can be used interchangeably.

When a man fails to understand a fundamental distinction like this, his opinion on the minutia of the question is worthless. Dr. Hall never, it seems, thought of any waves except those of gravity. These have no points of "condensation" or "rarefaction." It becomes a curious question as to what conception those words must arouse in Dr. Hall's mind when he sees them in scientific books. If we fail to see this distinction, all the physicists say concerning wave-motion and the propagation of sound is but a jumble of words; seeing the distinction, the meaning is clear.

Keeping this distinction in our minds, let us look at some quotations :

> "In water-waves we distinguish carefully between the motion of the wave and the motion of the water which forms the wave; so here, the wave-motion is totally different from the motion of the air itself. The wave, *i. e.*, the state of condensation and subsequent rarefaction, travels swiftly forward; but the masses of air, which suffer these condensations and rarefactions, simply *tremble in the line of that motion*."—*Olmsted's College Philosophy*, p. 187.
>
> "By a wave of sound is meant an undulation, or wave-like motion, communicated to the particles of air in consequence of the vibration of

the sounding body. It consists of two parts, one in which the air is condensed, called a condensation, and the other in which the air is rarefied, called a rarefaction. Whilst the undulation or wave is transmitted through the air, the aërial particles themselves have but a very small motion to and fro, in the direction in which the sound is propagated, Such oscillatory movements are necessarily attended by con densations and rarefactions."—*Lee's Acoustics, Light, and Heat*, p. 129.

"The motion of any particle is backward and forward in the line of propagation, and not 'up and down' across that line, as in the case of water-waves."—*Avery's Elements of Natural Philosophy*, p. 372.

"He [Newton] has explained the progression of sound by an undulatory, or rather a vermicular motion in the parts of the air. If we have an exact idea of the crawling of some insects, we shall have a tolerable notion of the progression of sound upon this hypothesis. The insect, for instance, in its motion first carries its contractions from the hinder part, in order to throw its fore part to the proper distance, then it carries its contractions from the fore part to the hinder, to bring that forward. Something similar to this is the motion of the air when struck upon by a sounding body."—*Nicholson's Encyclopædia*, art. "Acoustics."

"The vibrations of the particles, in an undulation of this kind, are called *longitudinal*; and it is by such vibrations that sound is propagated through the air."—*Deschanel's Elementary Treatise on Natural Philosophy*, p. 876.

"Waves are *longitudinal* or *transverse*, according as the particles vibrate in the same plane with the path of the wave, or across it."—*Gage's Elements of Physics*, p. 431.

I also ask the reader to examine the list of quotations given on pages 78 and 79 of Dr. Hall's book.

As we shall see, there is no fixed ratio between the velocity of particles of a wave and the velocity of the form of a wave. This thought is still in a chaotic condition in Dr. Hall's mind.

The Doctor has wasted a great deal of printer's ink in telling the world that he does not know that the term "water-wave" is a generic, not a specific term. (As an illustration, see page 338.) The term "water-wave" is generic, and applies to any "gravity wave." It indicates the kind of wave, not the material in which it moves. The German still calls the railroad the *iron-way*, though it is no longer made of iron, but of steel, and probably always will, whatever the material of which it may be made.

What should be thought of the logical acumen of a man who, in some coming generation, should try to prove that we in this latter part of this nineteenth century still used wooden plows because, forsooth, we still used the word "mold-*board*" when describing the parts of a plow? It would only suggest that this part of a plow was once a board. So waves were first studied in water, but the term "water-wave" has become generic. "Water-waves" may run in water, molasses or mercury, and if mighty billows roll along the upper surface of the atmosphere they are still water-waves.

Likewise has the term "air-wave" become generic. "Air-waves" may run in air, water, lead, brass or any other material. Yet the Doctor tells us that if an air-wave (that is, a wave in air) is transmitted to iron it becomes an iron-wave; if, then, it is transmitted to a rock it becomes a rock-wave, and so on, *ad nauseam*.

Dr. Hall has spoiled a great many pages of nice white paper trying to make sensible people sick by putting such stuff upon it. But it may be asked, "What are you going to do with the phrases 'precisely similar nature,' 'essentially identical' and 'exactly in the same way'?" I reply, "I am not going to do anything with them; they do not trouble me." I would say, however, that they are not taken from the original. Prof. Helmholtz writes his books in his own language—the German. I have never seen a copy of the "Sensations of Tone" in the original, and can not say whether the translation is correct or not. I do not suppose Dr. Hall had ever seen such a copy when he wrote his book, and as neither of us know, we will assume that the translation is correct. Prof. Helmholtz simply stated the analogy, taking it for granted that every one who would read his book knew that sound-waves belonged to "longitudinal" not "transverse" vibrations, and would know there must be a limit beyond which the analogy must not be pushed.

Take this passage from Nicholson's Encyclopædia, published in 1818:

> "The sonorous wave body having made its impression on the contiguous air, that impression is propagated from one particle to another, according to the laws of pneumatics. A few particles, for instance, driven from the surface of the body, push or press the adjacent particles into less a space; and the medium, as it is thus rarefied in one place, becomes condensed in the other: but the air thus compressed in the second place is, by its elasticity returned back again both to its former place and its former state, and the air contiguous to that is compressed; and the like obtains, when the air less compressed expanding itself, a new compression is generated. Therefore, from each agitation of the air there arises a motion in it, *analogous* to the motion of a wave on the surface of the water, which is called a wave or undulation of air. In each wave the particles go and return back again through very short equal spaces, the motion of each particle being *analogous* to the motion of a vibrating pendulum, while it performs two oscillations; and most of the laws of the pendulum, with very little alteration, being applicable to the former."—Art. "Sound."

The writer here says a sound wave is "analogous" to a water-wave. He also says the motion of a particle in a water-wave "analogous" to the motion of a pendulum.

Suppose some one would say, "The pendulum moves in a line almost horizontal, therefore the particle in a water-wave must move in a line almost horizontal, because this writer says the two movements are analogous." Would we all be convinced of the conclusiveness of the argument?

The writer in the Encyclopœdia simply took it for granted that the reader would not lay aside his common sense when he read his article, and Prof. Helmholtz did the same thing.

The Doctor says on page 237 that it is "essential" to his argument that there shall be no difference whatever between water waves and sound-waves. But we have seen that one is a wave of transverse vibration, the other of longitudinal.

A large part of Dr. Hall's book is but ringing the changes on three phrases—phrases of such obvious meaning that Dr. Hall can claim the distinction of being the first man who was ever misled by them, and no one will not dispute his claim.

There seems to be a mental infirmity in Dr. Hall, such that it is impossible for him to see the difference between a transverse and a longitudinal vibration. Thus:

> "But if such a pitch of sound as I have assumed passes through iron in this way, having an actual wave-length of 476 feet, and a depth of 'amplitude' so 'infinitesimal' that the most powerful magnifying glass fails to reveal it, then how much, I ask, does it lack of a straight course? If a line were drawn 476 feet, so nearly straight that a powerful microscope could not reveal the least deflection, is there a mathematician on earth who would not, without a moment's hesitation, pronounce that a straight line? Am I not justified, therefore, when I assert that so far from sound passing through rock, iron, water, wood, or even air; by wave-motion (which has no existence at all without amplitude), its route can only be a direct line?"—p. 245.

If the amplitude in a *longitudinal* wave were ten feet, the particle would not depart from a right line.

For a man to stuff the phrase, "precisely similar nature," into one ear, "exactly in the same way" into the other, take "essentially identical" on his tongue, shut his eyes, and be forever afterward absolutely impregnable to every argument, proof, statement, explanation, and new conception is with parallel in the annals of Psychology. And if, when his error is pointed out to him, the blunderer again mumbles over his pet phrases as a pleading excuse for his mistake, we could scarcely avoid the conclusion that the man had become a monomaniac.

All this part of Dr. Hall's book is a great pyramid, standing on its apex, and that apex an analogy, forced to "go on all fours."

Another question that the Doctor asks, is this: How great is the amplitude of a sound-wave?

He gets excited about this. He craves an answer as a favor, and demands it as one of his inalienable rights. Scientific men are base deceivers for not telling him.

Now, the question is just as sensible as this, and no more so—How large are the waves when a stone is thrown into the water?

We reply. It depends on three things—the size of the pebble, the distance through which it falls, and the distance from the center of the circle at which we measure the expanding wave.

Pebbles are not all of the same size, and the size of the waves will vary with them. Also the waves are larger the farther the pebble falls. If we measure the wave after it has run twenty feet, the altitude is not so great as though it had come but ten feet. Sound-waves, even at the source, are not all of the same amplitude, for loudness depends almost entirely on amplitude, but all sounds are not equally loud at the source. But the water-waves, as they spread into larger circles, become less in amplitude, so also sound-waves, as they spread into larger spheres, become of less amplitude, and their loudness diminishes.

Dr. Hall's question admits of no definite answer, and physicists do not trouble themselves about it.

In the second chapter I have given Lord Rayleigh's calculation, but we see he took into consideration the pitch of the sound and the intensity at the origin.

In the light of these facts this passage is not very formidable:

> "Now, suppose I should ask Prof. Tyndall to tell me the exact, or even approximate *amplitude* of the vibrating air particles in feet or inches for this system of waves, which is here shown to have a determinate wave-length of 'four feet four inches,' could he do it? I answer, emphatically, he could not, and, if he could, he dare not to, for it is a notorious fact that though these writers on sound are constantly calculating and recording the 'wave-length' in literal '*feet*' and '*inches*,' of tones of various degrees of pitch, *they have never once, in all their writings, so much as intimated even the approximate amplitude or width of swing of the air particles in any single system of sound-waves.*" (Italics his.)—p. 239.

On page 323 Dr. Hall says:

> "There is no escape from this rule, as the reader, no doubt, sees; for this *contraction of wave-length*, and this *diminution of velocity*, according to the ratio of *decrease in amplitude*, is strictly and philosophically interdependent and coincides with the laws of wave-motion, as here evolved." (Italics his.)

It is easy to show that the Doctor "evolved" his law out of his own inner consciousness, and not out of his experiments.

Allow me to lay down two laws which flatly contradict this which the Doctor has formulated, and the reader shall judge as to who is correct.

(1) *A wave once started maintains its wave-length unchanged*, however much the amplitude may diminish.

(2) The velocity also is a constant quantity. *A wave once started maintains its velocity unchanged*, however much the amplitude may diminish.

As a personal favor I ask the reader to try throwing some pebbles into a still pond and watch the wavelets as they spread into ever widening circles.

The amplitude incessantly diminishes, but the crests maintain the same distance between them as far as the eye can trace them.

Neither does the velocity become slower as the amplitude dininishes; the little wavelets, when they have grown so small that the eye can scarcely follow them, are running as fast as they did near the center of the disturbance.

The Doctor looked on the placid waters of the bath-tub and the tiny wavelets running on the surface. But he saw only that which a child ten years old would have seen. He saw the circle of the wave get larger and the amplitude get smaller. That is absolutely all he saw.

Each wave was about an inch from crest to crest—that is, in wave-length. After the wave had traveled about the full length of the tub he wondered how far it then was from crest to crest. But he did not look. He had reasoned it out before and that was enough for him.

If he had looked he would have seen that the distance was the same as at first—an inch. He had reasoned it out that this distance should be constantly diminishing.

Speculation had told him that the velocity of a wave should steadily grow less as the amplitude diminished.

Here was a chance to test his theory, but he did not do it. Thus this initial experiment taught him nothing.

He dropped a pebble into the secluded mill pond. The same law of Nature prevailed. But he saw not that a wave once started maintained a constant velocity and wave-length, that the amplitude alone diminished. He was bent on getting a different answer.

With his friend he gazed on one of the watery highways. The same law prevailed for larger and smaller waves.

He visited Rockaway Beach,

> "On old Long Island's sea-girt shore,"

And "What were the wild waves saying?" They were saying in thunder tones that the amplitude alone of a wave varies—that (except in the case of breakers) the wave-length and velocity are constant.

On page 245 the Doctor says a wave 476 feet from crest to crest, and only one inch high, would be a "ridiculous caricature on wave-motion." Then Nature perpetrates "ridiculous caricatures" on herself. The ratio in such a wave between the altitude and wave length is as 1 to 5,712. As the length remains constant while the amplitude diminishes, any wave, if it runs far enough, will come to this ratio.

The sea is sometimes found to be in very long, flat, smooth waves. Sailors tell us that these are caused by a storm at some great distance. The waves have run beyond the direct influence of the wind, and their amplitude has diminished but their wave length has remained unchanged. Hence they will come to this very ratio, but Dr. Hall says this ratio is a "ridiculous caricature on wave motion." The wave is responsible, not the physicists.

The amplitude diminished and Dr. Hall says the wave-length should contract at the same time, hence a storm at sea should end in the far distance in a sea broken up into ripples!

Dr. Hall had a case to make out, hence he proceeded to formulate laws suiting his theories however flatly his experiments contradicted them. What a fiasco is this:

> "If, therefore, there is the least analogy existing between actual wave-motion, as thus emplified, and sonorous propagation, it must be perfectly clear to a logical mind that a *sound should travel slower and slower the farther it gets away from the generating instrument, while it should also become higher and higher in pitch by the contraction of its wave-lengths, as this is exactly the manner in which water-waves are propagated.*" (Italics his.)—p. 323.

And all this unconscious burlesque on taking scientific observations by the High Priest in the temple of the new dispensation in science.

Dr. Hall, in his book, discusses but a few of the known experiment and phenomena in acoustics. And even were his criticism in each case correct, the vast majority would remain unanswered and unexplained. But it is not necessary that this monograph be expanded beyond the limits of the book it reviews.

CHAPTER X.

THE CONSERVATION OF ENERGY.

"Thus the seer,
With vision clear,
Sees forms appear and disappear,
In the perpetual round of strange,
Mysterious change
From birth to death, from death to birth,
From earth to heaven, from heaven to earth;
Till glimpses more sublime
Of things, unseen before,
Unto his wondering eyes reveal
The Universe as an immeasurable wheel
Turning for evermore
In the rapid and rushing river of Time."
—LONGFELLOW.

The Ancients believed that when water evaporated or wood was burned, the matter was annihilated.

Moderns believe that matter, in the ordinary course of Nature, is indestructible; that there is as much matter in the Universe as there ever was.

We may call this "the conservation of matter."

The Ancients believed that one elementary substance might be changed into another.

The philosopher's stone, which would change everything it touched into gold, was the phantom of their lives.

The Moderns believe that the elements are unchangeable, that no one is ever changed into any other.

The Ancients believed that there were many gods. They had one for each department of Nature, or even each locality.

Christians believe that there is but one God for all departments and all localities of the universe.

They believe, further, that this unification exalts their conception of God; that to divide God is to degrade Him.

The Ancients drifted to the belief that there were many elements. Moderns believe that there are but comparatively few—about sixty-eight. They believe that the wonderful diversity of Nature comes from diversity of combination, not diversity of elements.

The Ancients believed that there were as many types in the animal and vegetable kingdoms as there are species.

They further believed that the type of one species had little, if anything, to do with the type of any other species. Their gods were capricious, and constructed a new animal as the impulse prompted them, and without any thought of the other species in their minds.

Had such been the Process of Creation, to try to find unity of plan in Nature would be folly.

Moderns believe that there are but few types, and that the Creator has delighted to produce modifications of these for different purposes.

There is, undoubtedly, a structural analogy between the arms of a man, the fore legs of a horse, the wings of a bird, and the pectoral fins of a fish.

We believe that our God has not been capricious, but a Deity of continuity of thought, working according to a plan, and hence could produce a "universe"—a turning into one.

We believe that in tracing this unity in nature we can exclaim, in the words of one of the greatest of earth's scientists: "O God, I think Thy thoughts after Thee."

The Ancients believed that force could be annihilated, that energy is destructible.

Perhaps the most sweeping generalization that science has ever made is known by the name of "The Conservation of Energy."

It means that energy is conserved, is never annihilated, and that the Universe contains as much energy as it ever did. It is a recent generalization of science. It is a doctrine based not on metaphysical considerations, but is the result of countless experiments. It gives coloring to all scientific thought to-day.

One of its first practical applications was made in the perfecting of the steam engine. Calculations were made as to the amount of energy generated in the burning of one bushel of coal. The energy yielded by the engine was found to be far less than this. It was said, "There can be no destruction of energy. The disappearance must be due to wastage, not annihilation."

There arose among manufacturers a sharp competition as to who could build an engine that would make available the largest percentage of the energy given off by the burning of the coal, and it led to great improvements in steam boilers.

Men are trying to make another similar application of this famous doctrine that will be of great commercial use to mankind, while the mind is at the same time impressed with the belief that we do not live in a haphazard world.

An engine, say of ten horse-power, is used to drive a dynamo (a machine for converting mechanical energy into the electrical current). This current is then conducted by wires to a motor (a machine for converting the electrical current into mechanical energy).

But it is found that the horse-power of the motor does not equal the horse-power of the steam engine.

So it is said, "This loss must come from wastage, not destruction. The conversion must be made more complete, the leakage prevented."

When this is accomplished the engine and dynamo may be placed where coal and water are easily obtained, and the motor and factory where the circumstances of commerce demand.

In that day cities may be kept much cleaner than now. Much has been accomplished, but the conversion is not yet complete enough to make the invention capable of wide commercial use.

Science believes that matter is indestructible, and non-convertible. It also believes that energy is equally indestructible, but that it is convertible.

Conceive in the mind of a cubical glass box, six inches on a side, filled with bees. Push the box, which is lying on the table, two inches toward the South. This is "mass motion" of the box. The quivering cluster of bees within represents its "molecular motion."

In their present state suppose we let their motion represent heat. We blow smoke into the box. The bees become nervous and their quivering changes its character. We may let it represent electricity. We introduce more smoke. The bees become still more excited and the vibration of the shuddering mass again changes its character. This may represent light. Yet the box, all this time, has been lying perfectly still upon the table.

Modern science thinks it probable that heat, electricity, magnetism and light are "modes of motion."

But they are modes of "molecular" not "mass" motion. When it is said that probably the wires of the electric telegraph are in vibration, it is not meant that they are surging back and forth on the insulators to which they are loosely attached.

That would be "mass" vibration. The difference between "mass" and "molecular" vibration is not a mere difference in degree. There is a radical difference in kind.

The amplitude of the "molecular" might even be the greater.

The amplitude of sound-waves is, in the majority of instances, doubtless infinitesimal. But that does not make the vibration "molecular."

I do not think any one can study heat, electricity, magnetism and light for any considerable length of time without concluding that there is a vital connection between them.

Thus we see "Unity in Diversity" in *forces* as well as in *organisms.* One can hardly be produced without generating some of the others also.

A copper wire, which is a good conductor, joins the poles of a battery. The current, as shown by a galvanometer placed in the circuit, is strong. We cut the wire and connect the sections by a short piece of platinum wire, which is a poor conductor. When the current again passes the platinum wire is heated, but the strength of the current is reduced.

If the brakes of the railroad train be set so tight as to stop the wheels, the rails are heated by friction. But the "molecular" motion of heat is produced at the expense of the "mass" motion of the train.

The platinum wire seems to introduce something analogous to mechanical friction. If the original current is strong enough the platinum wire will yield light as well as heat. This is the principle of the "incandescent" electric lamp. They are all examples of the transformation of energy. Molecular motion is doubtless inconceivably complex.

The telescope reveals a complex system of planets, satellites and comets revolving about the sun, which is itself in motion. Yet our solar system may be but a molecule in the vast Universe of God.

A microscope of transcendent power might reveal as wonderful a system of "wheels within wheels."

The microscopic world may be as intricate as the telescopic.

The inconvertibility of matter stands forever in the way of any theory based on corpuscles explaining the convertibility of energy. The convertibility of motion makes such a conversion possible.

A motion might easily change its direction, a substance could not change its identity.

On one theory we can see how the electric current, passing freely in the copper wire, might, under other conditions in the platinum wire, be changed into heat, or even into light. The other theory says that such a transformation is impossible.

I think no one can study the telephone of Prof. Bell in its original form without concluding that the sound is changed in the transmitting instrument into an electrical current, and back again into sound in the receiving instrument.

I have said there is a radical difference between "mass" and "molecular" vibration.

So here the "mass" vibration of the diaphragm of each instrument acts as a sort of connecting link between the "mass" vibration of the sound and the "molecular" vibration of the electric current.

The men who have made the most wonderful discoveries of fundamental principles, and those who have made the most useful applications of these principles in electricity, heat, light and magnetism, have been men who favored the doctrine of vibrations and who were fully convinced that sound, at least, is a "mode of motion."

And though the theory of emissions has tried, for the last ten years, to dazzle the world with its effulgence, no man who has accepted its doctrine of sound has ever added one fact of observation or experiment to the stock of the world's knowledge on this subject, or has given to the world one useful invention based on his own theory.

CHAPTER XI.

CONCLUSION.

There is scarcely a thing in life to which men may apply themselves which can show more self-sacrificing devotion than science. Faraday gave up a fortune that he might give himself wholly to investigation; and died poor, but without regretting that he had made the choice he had. The scientists of the world are, as a class, men of moderate means. Original experimentation is mostly carried on at a financial loss to the experimenter. Many scientists devote the whole of their surplus income to making collections and trying experiments, sometimes, indeed, denying themselves many of the comforts of life that they may do this work, and attend the meetings of the various scientific societies. "Substantialists" have declared modern science to be heartless and to drift wholly toward selfishness. Many English literary men, politicians and ministers have lectured in America and carried some United States dollars home in their pockets. So American lecturers in England think a large collection of coins with the Queen's image upon them is a nice memento of their journey. The "substantialists" consider Prof. Tyndall the High Priest in this modern Temple of Selfishness. The proceeds of Prof. Tyndall's lectures in the United States amounted to $13,033. After the course was finished he made this announcement:

> "As an evidence of my good will toward the people of the United States, I desire to devote this sum to the advancement of theoretic science and the promotion of original research, especially in the department of physics in the United States."

The trustees allowed this sum to accumulate by the constant addition of unappropriated income until it amounted to $32,000. Then three fellowships in physics were established; at Harvard College, at Columbia College, and at the University of Pennsylvania.

The discussions that have taken place over various points in science, while they have often been long and eager, have been remarkably free from bitter personality. Religious polemics should have set the world a good example in this, but they have not.

There may not be so many protestations of love and affection between two scientific men who engage in a discussion on some point in their specialty, but there will not be half so many charges of dishonesty and insincerity either.

Scientific men have shown an unusual openness to conviction. It is not difficult to find examples where an advocate on one side of a question, at the beginning, has been converted and has ended by being a disputant on the other side. Such cases in religious or political discussions are rare. For political discussions are, to-day, far more dishonest than religious disputes ever were.

In his book Dr. Hall has simply set up a man of straw and gallantly knocked him down again. He is overwhelmed with amazement that the scientific world does not stand agast at the catastrophe and rush to the rescue as soon as it can regain the use of its faculties—that is, what few it has.

The straw man that Dr. Hall knocked down the scientific world did not set up, and it had no interest in his fate. To produce a theory outwardly similar to the wave-theory, but radically different on essential points, and then overthrow this, is not to overthrow the undulatory theory of sound.

Not that I thought this ancient doctrine was in any danger have I written this review, but I have written it because I thought it a good subject on which a young man might exercise his inexperienced pen.

Dr. Hall insists that there can be but two theories of sound, the undulatory theory and his own hypothesis—that is, that sound is composed of corpuscular emissions. At first he allowed this to mean much the same as material emissions. However, seeing the weakness of this theory, he has refined and explained away the meaning of the word "substance" until it means nothing thinkable, nor conceivable.

Hence the Doctor should have granted that there could be three different theories of sound. This apparent oversight, however, is really not such, but has a philosophy behind it. If, at first, a theory of sound, in which the emitted corpuscles had no physical or thinkable properties, had been presented to Dr. Hall, he would promptly have rejected it as fanciful. Though now advocating it, he left no place for it in his dual classification.

We have seen that no theory of emissions will do. The more such a theory is studied the more unreasonable it appears. This leaves us only the wave-theory, even though it contains some difficulties. Our work seems to be to find an explanation of the difficulties rather than to find an hypothesis that contains nothing hard to understand.

The "substantialists" were at first very anxious to gain the endorsement of some prominent physicist, but finding they can not gain it they are making a virtue out of a necessity and frequently talk as though they do not care about it; indeed, do not want it. They rejoice in the endorsement of the common mind. Mark Twain engraved a map of Paris, of which he was very proud, and, concerning which, he says:

> "Inasmuch as this is the first time I ever tried to draft and engrave a map, or attempted anything in any line of art, the commendation the work has received and the admiration it has excited among the people have been very grateful to my feelings. And it is touching to reflect that by far the most enthusiastic of those praises have come from people who know nothing at all about art."

Probably nothing else has been a source of so much amusement to the "substantialists" as the supposed fickleness of scientific men. Theories have been framed, they say, only to be discarded. They regale in the fact that theory after theory in geology has arisen, flourished and passed away. Now a theory of sound is brought to them which has been studied so little in the past that we may consider it brand new. It is very revolutionary in its character and will lead to the revision of all scientific theories. In this great work all can find enough to do to make them famous for all time to come. They examine it, can not convince themselves of its correctness, and so decline to become its adherents.

Then we read long articles on the conservatism of scientific men. "Substantialists" say of themselves that they must wait for another generation before they can be appreciated. Scientific men, we are told, are so conservative that no radical change occurs within the life-time of one generation.

Is not this instantaneous transformation of the former fickle gallant into a lover so constant that it amounts to absolute blindness, one of the most wonderful occurrences recorded in history?

This charitable explanation of the fact that no prominent physicist has accepted the corpuscular hypothesis satisfies some "substantialists," but seemingly the greater number think the true reason is that scientific men are so completely lacking in common sense that they can not understand plain argument, or else they do understand, and are convinced, but are too dishonest to acknowledge it. But it is not wise for a man to conclude that his opponent in argument is either a fool or a knave. It is even possible that a man may be mistaken himself sometimes.

When a little coterie of men find that they can not convert the world to their own way of thinking, it does not convince us of the correctness of their views for them to conclude that all the rest of the world are mentally infirm—that wisdom is found only in themselves. I once heard of a jury that disagreed. The vote stood eleven to one; and that one afterwards remarked that he "never saw eleven such obstinate men as those fellows were."

The writings of the "substantialists" are filled with misrepresentations, though in a large majority of cases the dishonesty is entirely unconscious. Take this, for example.

Speaking of the rise of scientific thought in the middle ages, Rev. J. W. Lowber says:

> "The old school of scientists became very much alarmed at the new awakening. They could readily see what would, in the near future, be their doom. So they made use of the civil and ecclesiastical powers to check the progress of the new doctrine. Such men as Copernicus and Galileo were rushed away to prison, and Luther was met with the most determined opposition. The excitement became so great that the fanatical rulers established that infernal court—the Inquisition. Racks, chains, scourges and thumb-screws filled its execution rooms, and the work carried on in the chambers of this horrid tribunal was calculated to strike terror to the bravest hearts. No tortures invented by untutored savages could compare to the blood-curdling horrors perpetrated by these inhuman monsters upon their victims. All Europe was, for a time, drenched with blood, but 'truth crushed to earth will rise again.'"—*Scientific Arena*, October, 1886, p. 71.

The men who persecuted the investigators of nature during the Middle Ages did not belong to "the old school of scientists." They were not "scientists" in any sense of the word, and did not claim to be. They would have scorned the name of "scientists."

They belonged to Rev. Mr. Lowber's own class—they were theologians, and gloried in the name. They opposed the new views in science, not because they conflicted with the science to which they had been accustomed, but because they conflicted with their theology. And there was no subterfuge about it; they said their opposition was theological. They were theologians first, last, and all the time, and they did not try, nor pretend to try, to bring scientific arguments to put down the new views. If any views conflicted with their views in theology, that was enough for them. That settled the question in their minds. This attempt by Rev. Mr. Lowber to distort the facts of history will not do.

The writings of the "substantialists" have been filled with declarations that "current science" is thoroughly worldly, and is of the earth, earthy. Time and again have they expatiated on the fact that among the last of Charles Darwin's works was that upon worms. The "substantialists" have been proud to say they will never trouble themselves about worms, nor any other trifles.

Their articles are written in the "blue azure" style. They have soared through space and delivered grandiloquent orations on grand themes, innocently supposing they were adding something to the sum of human knowledge. Though claiming to be "scientists," it is only occasionally that we can find among their writers any one who seemingly has ever tried any experiments at all.

Some of the greatest men in the history of metaphysics have vigorously denounced the experimental philosophers, saying they were debasing the mind, that they were abandoning the reflective powers and depending on the perceptive. This feeling, more than anything else for a long time, kept science in the background in our colleges. The other departments would not admit that the intellectual work in the scientific department was on as high a plane as that of their own department.

Science holds a prominent place to-day, but every inch of the ground has had to be fought for, to be wrenched from men who thought science was wasting its time on trifles. The "substantialists" have thought it unworthy of the human mind, with its great destiny, to deal with worms and other trivial things.

Any man who feels that way about the common things and phenomena about him will never do anything great in science, for greatness in science is based on paying attention to seeming trifles.

Rev. John P. Newman, in speaking of the Ancients, says:

> "Pythagoras at Crotona, Plato on the Promontory of Sunium, Cicero in his Tuscan Villa, thought it unworthy to seek the supply of man's vulgar wants. Astronomy is older than the pyramids, but the astronomers of the best civilizations of the past considered it a degradation to apply the science of the stars to the common wants of man.
>
> "They pursued the study of the heavenly bodies for the brilliancy of the thoughts suggested, and for the vastness of the universe it unfolded to the mind. Mathematics were esteemed useful for their mental discipline and for a certain spirituality they imparted. But they were not to be applied to those useful inventions that lift the burden of toil from the shoulders of humanity. Plato remonstrated with his friend Archytas because he had framed machines for useful purposes on mathematical principles, and Archimedes was half-ashamed of those inventions which were the wonder and admiration of the foes of his country."

Paying attention to seeming trifles leads to greatness in science, and paying attention to the world we live in, may even lead to God Himself. That marvelously gifted man, Rev. F. W. Robertson, says:

> "To ask, 'Why?' is the best Christian lesson for a child. Not the '*why*' which is the language of disobedience, but that '*why*' which demands for all phenomena a cause. It was this which led Moses on Mount Horeb to say, 'I will turn aside and see this great sight, why the bush is not burned.' So it was that Moses found out God."

APPENDIX.

THE AFFINITY OF CERTAIN COMPOUNDS

AS EXPRESSED IN TERMS

OF

ELECTROMOTIVE FORCE

A THESIS

PRESENTED BY

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1884

(UNIVERSITY OF MICHIGAN)

PREFATORY NOTE.

When the following thesis was read, it was the expectation of the writer to extend his studies in this direction further; but circumstances have not favored his plans, and he now publishes his monograph, in the hope that it may come to the notice of some one who has both the means and inclination to enter upon this sort of an investigation.

With the exception of one slight change, it is published exactly as read before the examining committee; hence the language is severely technical, and it is not expected that persons with no special scientific tastes and training will read this appendix.

A THESIS.

The investigations narrated in the following paper were undertaken at the suggestion of Professor J. W. Langley, and throughout the work I have enjoyed the benefit of his counsel.

The relation of electrical to chemical forces has been a favorite subject of study with chemists and physicists for a long time, even with those who did not believe in the identity of nature in these two forms of energy.

As late as April 5, 1881, Prof. Helmholtz[1] in his Faraday Lecture advocates the doctrine that these forces are fundamentally identical. After discussing Faraday's law of definite electrolytic action and its results, he says:

> "Faraday very often recurs to this to express his conviction that the forces termed chemical affinity and electricity are one and the same. . . I think these facts leave no doubt that the very mightiest among chemical forces are of electrical origin."

It will be seen even in the following pages that chemical forces may be measured and expressed in terms of electrical measurement, without any computations or conversions whatever. This fact certainly does not prove the identity of the two forces, but it is an interesting fact and one not to be overlooked in making up one's mind on the subject.

This attempt to get some numerical data as to the relative strength of attractions in chemical affinities is not a late effort only with workers in physical science.

Tidy[2] enumerates six methods that have been employed by different investigators, and which I transcribe, giving the name of one or more of the principal experimenters by each method.

I. Affinity measured by reference to the specific gravity of bodies. (La Place and others.)

II. Affinity measured by the force of adhesion. (Guyton Morreau.)

III. Affinity measured by the amount of force required to effect the decomposition of a compound. The decomposition was effected in this case, first—by heat; secondly—by a superior affinity.

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[1] *Popular Science Monthly*, June, 1881.

[2] *Handbook of Modern Chemistry, Inorganic and Organic*, by Charles M. Tidy. pp. 14–19.

IV. Affinity measured by the time of combination. (Wenzell.)

V. Affinity measured by the combining proportions. (Kirwan.)

VI. Affinity measured by the electrical condition. (Thompson.) In this method the "electrical condition" is shown by the successive precipitations.

It is not within the scope of this article to enter into a discussion of the merits of these various methods; suffice to say that Prof. Tidy closes his review by saying:

> "Summarizing these facts we may note, first, that we have no means of measuring affinity absolutely, and secondly, but very uncertain means of estimating it relatively."

VII. Another way of approaching this subject not included in the above list was first indicated by Joule[1] in the following words:

> "I take a glass vessel filled with a solution of an electrolyte and properly furnished with electrodes; I place the electrolytic cell in the voltaic circuit for a given length of time and carefully observe the decomposition and the heat evolved. By the law of Ohm I then ascertain the resistance of a wire capable of obstructing the current equally with the electrolytic cell. Then by the law we have proved [i.e., Joule's law] I determine the quantity of heat which would have been evolved had a wire of such resistance been placed in the current instead of the electrolytic cell. This theoretical quantity being compared with the heat actually evolved in the electrolytic cell is always found to exceed the latter considerably."
>
> "The difference between the results evidently gives the amount of heat absorbed during electrolysis, and is therefore equivalent to the heat which is due to the reverse chemical combination or other means."

This method has of late been employed by several experimenters, prominent among whom is C. R. Alder Wright,[2] who in a series of articles has discussed the results obtained by himself and others.

Without doubt this is one of the most truly scientific methods that has yet been employed and the results obtained by it are very instructive, but it seems to me that the difficulties of manipulation would be very great and the errors of observation would be liable to be large.

VIII. Another method much used of late and probably more available than any of the preceding is to measure the heat evolved during the combination of elements.

Thus, the heat evolved by the oxidation of a metal (as determined by burning it in an atmosphere of oxygen) evidently gives some indication of the absolute force binding together the atoms of metal and oxygen.

Or, if we dissolve the metal in an acid the amount of heat evolved will evidently give an expression for the affinity between the acid radical and the metal.

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[1] *Philosophical Magazine*, [4] p. 481.

[2] See *Philosophical Magazine* for 1880-1-2.

This field has been cultivated industriously especially by Favre and Silberman, Berthelot and others, and extended tabulated results published.

IX. Still another method is the one employed by myself, and which, as far as I can learn, no one has hitherto used. The principle of it may be explained as follows:

The atoms or groups of atoms of a molecule are held together by a certain force of chemical affinity. This attraction, we know, can be overcome by the electrical current and the atoms torn asunder. We also know that below a certain electro-motive force this separation does not occur. Now could we measure the electro-motive force of the current at the moment this separation first takes place, we would, evidently, have an expression for the strength of the chemical affinity which has been overcome. But if the battery current be now broken and the wires from the electrodes be connected, we find a reverse current generated by the recombination of the separated elements, and we may rationally assume that its electro-motive force is just equal to that which was necessary to separate the elements. But it must be remembered that a molecule is not always broken up into the individual elements that compose it, but often into groups of atoms only, when, of course, the electro-motive force of the polarization current will be different from what it would be in the first case. The fact is, when this proximate analysis, as it might be called, occurs, rather than the ultimate, the electro-motive force of the polarization current will be lower, for were the latter action the easier it would occur first.

Again, were the elements, or Ions, as they are called, able to unite with the electrodes at the moment of separation, evidently there would be no current of polarization at all, and any such action that may occur will have the effect of reducing the electro-motive force of the current.

I will now describe the various parts of the apparatus employed, how they were put together, and the methods of manipulation.

I. THE BATTERIES.

a. The battery employed was a new one, invented by MM. de La Sande and Chaperon.[1]

As I used it, it was essentially as follows. The battery in appearance and action resembles the common gravity battery, but, in place of the solution of copper sulphate the cell was filled nearly to the top with a saturated solution of commercial concentrated lye.

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[1] For various forms of this cell see *The Electrical World*, September 1, 1883.

This dissolved the Zinc, which was suspended as in the gravity cell. But in place of the copper plate of this latter cell a copper or iron dish was used, being filled with the black oxide of copper, which yielded up its oxygen to form water with the hydrogen evolved at the zinc plate, and thus prevented polarization of the battery.

Air must be excluded from the solution to prevent its absorbing carbon dioxide from the atmosphere of the room and thus becoming neutralized. This is easily accomplished by setting up the battery quite warm and scattering pieces of paraffine on the liquid, which melt and spread over the surface, and, on cooling, form an air-tight layer.

The inventors give the electro-motive force of this cell as .98 of a volt, though, as will be seen farther on, it was not necessary for me to know either the electro-motive force or the internal resistance; the latter, however, I know to be quite low.

This battery I found to be a very serviceable one, giving a current that was very constant, though often kept on a closed circuit for a whole afternoon, while it could be left on an open circuit for seemingly any length of time.

b. For measuring the difference of potential between the ends of the Rheostat wire I used the well known Clark's Standard Cell, in a way to be described. Before my investigations were quite finished some sort of decomposition seemed to occur in the paste of this cell, so that I did not place much reliance on the accuracy of its electromotive force, as the different cells did not agree among themselves. This unfortunate circumstance vitiates the numbers given for silver and mercury especially.

II. THE GALVANOMETERS.

a. The galvanometer *g*, page 11, was a fine astatic needle instrument. It was very delicate, as I found by various tests that I applied.

b. The one marked g' was an ordinary instrument, not at all delicate, and kept in the circuit all the time to detect any change in the strength of the current during the progress of an experiment.

III. THE RHEOSTAT.

This will be readily understood by referring to the figure on page 13. A board about 40 centimeters wide and something over a meter long was planed smooth and strengthened by cleats placed on the under side and which also served as supports.

Across the upper side of the board two lines were drawn at right angles to the edge of the board and just one meter apart.

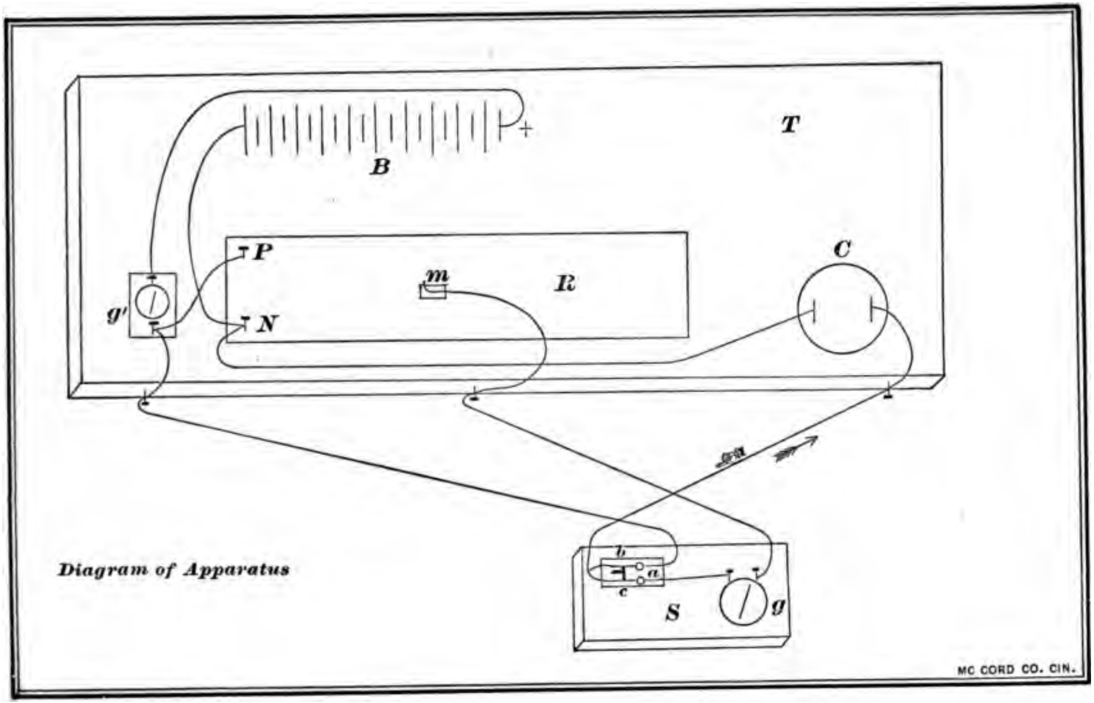


Diagram of Apparatus

The intervening distance was next divided into centimeters by lines drawn parallel to these first lines. A narrow strip of paper was then glued along one edge of the board and the lines numbered upon it from 0 to 100, beginning at the left.

The whole was at last covered with a coating of shellac varnish to prevent the absorption of moisture and consequent warping.

A number of bits of copper about two centimeters wide and three long were then cut from sheet copper. Ten of these were placed along the first line at the left hand side, with their inner edges just touching it, and 5 or 6 *m m* apart. Nine pieces were similarly placed at the other end of the board, but with their middle points just opposite the spaces of the other row.

Eighteen copper wires were stretched between these pieces of copper after the manner represented in the drawing (the number of wires being reduced however) and soldered fast at the ends. This wire was about 6.9 mils in diameter and had a resistance, according to my measurement, of .67 ohm to the meter.

To the pieces of copper *P* and *N* binding-posts were attached, which thus became the poles of the Rheostat.

By this arrangement I evidently had before me eighteen meters of wire, whose resistance in various parts of its length for equal spaces was equal, as I found by measurement.

Now suppose a current is flowing through the wire from the positive pole *P* to the negative pole *N*, and that the difference of potential between the two poles is three volts, as I have found by measurment.

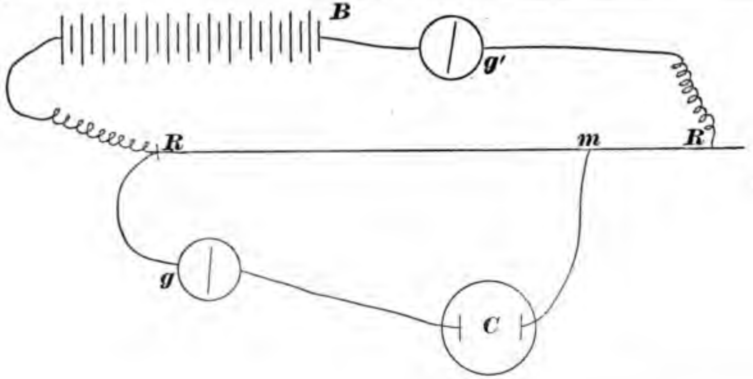
If I place a finger of one hand upon the negative pole and a finger of the other hand upon the wire at the end of the twelfth meter, counting from the negative pole, I have evidently taken off a part of the current which is flowing through the wire, and the electro-motive force of the part so taken off is two volts, for the difference of potential varies gradually from 0 at the negative pole to 3 volts at the positive pole, and as I have taken off two-thirds of the wire I have evidently taken off two-thirds of the whole electro-motive force.

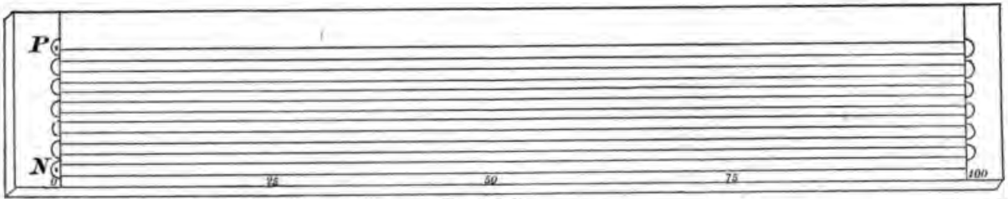
The movable block *m*, page 11, was for the purpose of making this sliding connection. As I could measure to centimeters, I could thus measure to the one eighteen-hundredth of the whole difference of potential between the poles.

IV. THE ELECTROLYTIC CELL.

a. For the cell itself a small beaker was used. This was generally set in a larger, thicker one and which was also slightly taller and

Theoretical Diagram





Rheostat

MC CORD CO. CIN.

thus could act as a support to the pieces of wood to which the electrodes were attached.

The space between the two beakers could be filled with ice or snow when low temperatures were desired.

When high temperatures were desired the cell was placed in a water-bath beneath which a lamp could be lighted.

Sometimes the bath itself was packed with snow around the cell, and which could thus gradually be raised from perhaps 1° C to 94 or 96° C.

I found that a liquid thus placed in a water-bath would generally not rise above 94° C.

At the altitude of Ann Arbor water boils below 100° C anyhow.

b. Platinum electrodes were generally used. These were thin plates, about 2 centimeters wide and 3 long.

One end of the plate was folded tightly over a thin platinum wire which, to give it rigidity, was passed through a very small glass tube, and this latter was fastened to a piece of wood for a support.

Before using the electrodes they were cleaned with acids, washed in pure water, and generally ignited for some time.

In the electrolysis of chlorides I found that the liberated chlorine united with the platinum electrode, hence, in working with these compounds I had to use carbon electrodes. These were sticks of common gas carbon something less than a quarter of an inch square. Before using them they were boiled for several hours in hydrochloric acid to remove any metals they might contain.

In working I did not measure the distance between the electrodes, but aimed to keep it about the same—something less than 2 centimeters.

The apparatus was set up as shown in the diagram on page 11, of which *T* is a long table on which were placed the battery *B*, the rheostat *R*, the electrolytic cell *C*, and the coarse galvanometer *g′*.

S is a smaller stand holding the fine galvanometer *g*, and the double key *a*.

The whole was connected as shown in the diagram, the wires running from one table to the other being covered with a coating of shellac varnish and fastened to the floor.

Now, supposing that everything is in readiness, the arm *b* of the double key *a* is depressed and the wire dips into the corresponding mercury cup; and this, it will be seen, sends a current into the cell in the direction of the arrow.

But if the other arm be now depressed (which at the same time makes the connection through the galvanometer) we find the current of polarization flowing through the wire in the direction of the hand. But the current from the rheostat is likewise flowing through the galvanometer in the direction of the arrow.

If these two currents be just equal they will neutralize one another and the galvanometer will not be deflected. By a series of tentative efforts, made by changing the position of the point where the block touched the wire, this neutral point was soon found.

The polarization current, however, maintained its strength but for a few moments, after which it rapidly ran down, hence, only the first throw of the needle could be considered.

At first glance it might appear that the galvanometer was used to compare electro-motive forces. But a little study will show that it was used to measure neither strength of current nor electro-motive force, but it was used merely to indicate the absence of any current, hence, the absence of any electro-motive force.

Knowing the difference of potential between the ends of the rheostat wire it was easy to compute the electro-motive force of the polarization current.

Thus: Let $a =$ Difference of potential.
Let $d =$ Length of wire, counting from N to M.

Hence, $\frac{a\,d}{18} = x =$ The E. M. F. taken off the rheostat, $=$ the E. M. F. of the polarization current.

In a similar manner the difference of potential between the poles of the rheostat could be estimated by placing one or more of Clark's Standard Cells in place of the electrolytic cell, so that the current from this battery flowed in the same direction as the polarization current did.

Now, Let $n =$ number of Clark's cells employed.
Let $d =$ distance on rheostat wire from N to M.

Whence, $\frac{1.457\,x\,n}{x} = d$, where $x =$ difference of potential.

I now desire to present a table of electro-motive forces of various substances as determined by this method.

When in the following table the name of the electrode is followed by the symbol of a metal, it means that during electrolysis the metal was precipitated on the negative electrode.

The numbers given are the result of single determinations. Doubtless the mean of many observations would give more accurate and satisfactory results. Most, however, if not all, were re-determined when I found that the numbers varied but slightly from those here given.

Name of Salt.	Kind of Electrodes.	E. M. F. at 16° to 20° C.	E. M. F. at about 94° C.	Per cent. of Fall of E. M. F.
$MgCl_2$	C & C	2.64	2.25	17
$CaCl_2$	C & C	2.77	2.17	21
NaCl	C & C	2.86	2.54	11
KCl	C & C	2.90	2.65	9
HCl	C & C	1.83	1.68	8
$BaCl_2$	C & C	3.00	2.59	13
$FeCl_2$	C & Fe	1.92	1.64	14
$MgSO_4$	C & C	2.90	2.39	18
$ZnSO_4$	Pt & Pt (Zn)	2.64	2.31	13
$CdSo_4$	Pt & Pt (Cd)	2.47	2.30	6
$FeSO_4$	C & Fe	2.01	.82	59
$CuSO_4$	Pt & Pt (Cu)	1.62	2.39	14
$HgSO_4$	Pt & Pt (Hg)	1.59	1.19	31
Ag_2SO_4	Pt & Pt (Ag)	1.25	1.04	17
$Cu(NO_3)_2$	Pt & Pt (Cu)	1.55	1.36	12
$(NH_4)_2CO_3$	Pt & Pt	2.22	1.65	25
$KHCO_3$	Pt & Pt	2.44	1.59	34
H_2O	Pt & Pt	2.13	1.86	12

AFFINITY.

So far I have used the word "affinity" in the popular acceptation of the term as meaning the attraction between the atoms of different elementary bodies resulting in the formation of new chemical compounds; and that this form of attraction, differing from gravitation, acts only at insensible distances.

But we know that the value of this force may be changed by varying the conditions.

Two views have naturally flowed from this observed fact.

First. We may look upon the atom us having stored up within it a certain inherent amount of attractive force which never changes, and that any apparent change arises solely from the fact that an opposing force has intervened, and that it is the excess over this only that we observe; or *Second,* We may look upon the atom as not having any absolute amount of attractive force peculiar to it, and that any such force that it does exhibit at any time is simply the resultant of the many conditions that surround it. But we desire a theory more dynamical in its nature, and which will yield itself to precise reasoning.

This affinity is evidently a form of energy; if so it may be expressed by the formula $\frac{1}{2}mv^2$—a function of mass and velocity (m, however, is a constant).

The relative mass of the atoms we know—their atomic weights—and these do not change.

The velocity we do not know, and it is, moreover, liable to change. Suppose, now, to any molecule a given amount of force is imparted. It may all appear as a vibration of the molecule as a whole—hence it will affect surrounding bodies, in fact it appears as heat, electric current, etc., or, the force imparted may only have the effect of setting up internal vibration of the component atoms, consequently will not affect surrounding bodies, in short it becomes latent heat, or potential energy.

As we have seen, the energy of this system of atoms is a function of its mass and the mean velocity of its parts.

The fact that no two atoms have the same weight would immediately suggest that no two atoms have the same inherent energy—other things being equal.

But we know that other things are not equal, for, the strength of affinity is not directly proportional to the atomic weight.

But it is possible that the mean velocity of the system should be so correlated to the mass that the energies of all elements should be equal.

But we know that such a correlation does not exist, for all atoms do not have the same strength of affinity. Thus we see that two quantities go to make up affinity—a constant and a variable. Hence we come to these conclusions:

I. It can not be said that an atom has a fixed total quantity of energy inextricably locked up with it, for a variable quantity is involved.

II. It can not be said that the energy (or affinity) of an atom depends solely upon the conditions that surround it, for a constant quantity peculiar to itself is involved.

Could an atom of each of the elementary bodies be isolated and placed under absolutely the same conditions that surround all the others, and the energy of each determined, we would have an expression for its energy or affinity compared with all the others.

Thus we see that two atoms may have the same strength of affinity, but not under the same conditions, as the constant factor involved (the atomic weight) is in no two cases the same.

Thus far I have used the words atom and molecule interchangeably, but perhaps a word on this point is necessary. It might be objected that while the principle applies well enough to molecules in which internal movements can take place, and hence latent heat exist, yet in the case of the monatomic elements there could be no such thing as latent heat.

If we look upon an atom as absolutely a unit, inflexible, not made up of parts, or, if so made up that the parts never change their relative positions, then, if atoms act as individuals only, the objection is a valid one.

And it is true that in the processes of the laboratory such seems to be their constitution,—they are never broken up into component parts.

But there are physical reasons for modifying this view, and it will be seen that in this discussion the words atom and molecule may be used interchangeably, for the atom of systematic chemistry itself becomes a molecule.

Now to apply these principles to the subject in hand.

Two atoms each have a certain amount of energy stored up within themselves. They combine to form a molecule. The internal vibration of each is reduced; but this energy appears as a vibration of the molecule as a whole; it is manifested as heat, electric current, etc.

To separate these two atoms again as much energy must be imparted to them as they gave up when they came together.

This energy may be imparted as heat and gives the phenomenon of dissociation, or, it may be imparted by the electric current and gives the phenomenon of electrolysis.

I desire now to refer to the work of well-known investigators in this field of measuring affinity, and among them there are none better known than Favre and Silberman. I take the data as collated by Sprague.[1]

But before comparing them with the numbers obtained by myself, a few words of explanation are necessary.

I. As I understand the experiments of these investigators, the substances were used in the solid state, excepting, of course, those which are gases at the ordinary temperatures.

But we know that considerable energy is evolved or absorbed in changing a substance from a liquid to the solid or to the gaseous state.

We have an illustration of the energy expended in changing a body from the solid to the gaseous state in the successive oxidations of carbon.

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[1] *Electricity; Its Theory, Sources and Applications.* By John T. Sprague.

$$C + O = CO = 30150 \text{ heat units.}$$
$$C + 2O = CO_2 = 96960 \text{ `` `` }$$

It is but reasonable to suppose that the affinity of the carbon for the first atom of oxygen is as great as it is for the second; and we see how large a share of the energy set free by the first combustion is expanded in changing the carbon from a solid to a gas.

II. As throwing some light upon the results obtained with salts in the solid state compared with those in solution, I transcribe some numbers given by Berthelot.[1]

Heat disengaged, in calorics.

Salt.	In Solid State.	In Solution.
KCl	105.0	100.8
NaCl	97.3	96.2
$CaCl_2$	81.1	93.8
$MgCl_2$	75.5	93.5
$FeCl_2$	41.0	50.0

To my mind these considerations do not explain why the numbers obtained by the method I used are larger than the numbers given in the books. But whatever may be the explanation of it the results I obtained are remarkably consistent among themselves, and one can place some reliance upon them in foretelling results.

III. Sprague generally gives the data in foot-pounds, but in the table to which I refer (p. 214) they are also given in "equivolts"—a term originated by himself. He defines it as "the force engaged in effecting one equivalent of chemical action in a circuit of one ohm resistance, and under the volt electro-motive force."

He defines a "chemic" to be a "rate of current which in a second is equal to .17606 of a veber, and would in ten hours deposit or set free one equivalent in grains of any element or iron."

The value of the "equivolt" is 4673 foot-pounds. It will be seen that his numbers express energy, while mine express intensity—the element of time is not involved. Hence for the purpose of comparison the equivolt may be taken as equal to the volt.

It will be seen that the chlorides are higher than the sulphates. This is because he makes an allowance for the formation of water when an oxide dissolves in HCl, but does not make this correction in the case of H_2SO_4, saying that the real energy of the formation of SO_3 is not known. Hence, for purposes of comparison, I have added a column (No. 4), deducting this correction from the chlorides.

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[1] *Essai de Mechanique Chemique* par M. Berthelot, Tome Premier, p. 378.

I.	II.	III.	IV.
Base.	Sulphates Equivolts, E. M. F.	Chlorides Equivolts, E. M. F.	Chlorides (adapted) Equivolts, E. M F.
Copper.	1.258	1.467	.770
Hydrogen.	1.919	1.707	1.010
Iron.	2.069	2.268	1.571
Zinc.	2.248	2.400	1.703
Sodium.	3.795	3.996	3.318
Potassium.	3.913	4.121	3.424

The data given in the table on page 16, together with the results of other experiments to be narrated in the proper place will, I think, leave no doubt as to the truth of the following propositions.

With each proposition I will give all the data bearing on the subject.

PROPOSITION I.

Substitutions between salts take place in the order of the relative magnitude of their electro-motive forces.

$MgCl_2$ 2.64	$FeCl_2$ 1.92
$MgSO_4$ 2.90	$FeSO_4$ 2.01
$Cu(NO_3)_2$ 1.55	NaCl 2.86
$CuSO_4$ 162	KCl 2.90

The inference from these data does not need any special exposition. In each case it will be seen that the sulphate has a higher electro-motive force than is found in either the corresponding chloride or nitrate. This is in accord with the well known fact in qualitative chemistry, that sulphuric is one of the strongest acids, and will displace most others from their salts.

This field of investigation will, I think, prove a very fertile one when cultivated more thoroughly by scientific chemists.

Some day, no doubt, elaborate tables will be made out, giving the numerical data (in E. M. F., or otherwise) as to the affinity between the elements of various salts, so that one can make out the reaction by examining the magnitude of the affinity, One must not forget, however, that the affinity alone does not determine the reaction.

For example, the relative quantity of the salts present has a large influence.

Thus if to a solution of equal parts of KOH and NaOH we add just enough HCl to unite with all the K, it does not all unite with that base, but part of it goes to the Na, and, neglecting numbers, we have a reaction something as follows:

$$KOH+NaOH+H_2O+HCl=KCl+NaCl+KOH+NaOH+H_2O$$

COROLLARY.

A metal can not dissolve in an acid unless the free acid has a lower electro-motive force than the corresponding salt of the metal.

This follows directly from the above proposition if we define an acid to be "salt of hydrogen," when it becomes simply a case of substitution between salts.

$CuSO_4$	1.62	$CdSO_4$	2.47
$HgSO_4$	1.59	$ZnSO_4$	2.64
Ag_2SO_4	1.25	$FeCl_2$	1.92
$MgSO_4$	2.90	H_2SO_4	1.90
$FeSO_4$	2.01	HCl	1.83

Here Mg, Zn, Cd, and Fe are soluable in H_2SO_4, while Ag, Hg, and Cu are not. The table also shows why iron dissolves in hydrochloric acid.

Why Ag, Hg, and Cu should dissolve in hot, concentrated sulphuric could, perhaps, be explained by a further examination of the influence of heat.

It will be noticed that at the boiling point the electro-motive force of the iron sulphate and chloride is lower than that of the corresponding free acid.

This, I think, can be explained as follows: The ferrous salts by being boiled in the open air were largely oxidized to the ferric condition; they gave every indication of such change.

Hence I was measuring the ferric salt, and the fact that its electro-motive force is lower than that of the corresponding free acid shows that a ferric salt can not be formed directly by dissolving the metal in the acid, for this will always form the ferrous salt, and the ferric condition is the result of an after change.

PROPOSITION II.

The electro-motive force of a compound falls with the rise of temperature.

An examination of the data given on page 16, shows that there are no exceptions to this law among the salts upon which experiments were made.

The phenomenon of "dissociation" would lead us to expect such a result as the table shows; and the principles involved in this phenomenon I have discussed under the head of "affinity."

The considerable per cent. of fall of some of these salts shows that, with them this breaking up occurs at quite moderate temperatures.

Could a curve be made out illustrating this decline and its equation determined, this point of dissociation might be calculated.

But very accurate measurements must be obtained to make out this curve with exactness; while the carrying of such curves illustrating physical phenomena beyond the point of actual observation is of doubtful propriety, as later and more extended experiments often prove that they have been very misleading.

However, I will give the results of three experiments I tried with reference to this point.

The cell, filled with a saturated solution of $CuSO_4$, was placed in a water bath, packed around with snow and NaCl, and the electromotive force measured as the temperature was gradually raised by a lamp placed beneath, Table I.

A similar experiment was tried with the cell filled with pure redistilled water, Table II.

I.		II.	
Temperature.	E. M. F.	Temperature.	E. M. F.
1¾°C	1.62	1°C	2.22
19	1.58	15	2.13
36½	1.51	31	2.09
54	1.45	44	2.06
69¼	1.42	54½	2.03
80	1.39	74	1.90
93	1.38	93	1.86

The intervals here are irregular; but they are constant enough for us to see that the curves for these two substances would not differ materially, though they make it probable that the curve for $CuSO_4$ would touch the axis of abscissas sooner than the other would.

The resistance of pure water was so great and current so reduced that the action was feeble and indistinct. So I used acidulated water, and otherwise modified the next experiment.

The water was placed in a U-tube and the wires from the electrodes hermetically sealed in the ends.

Also to raise the boiling point of the water in the bath surrounding the cell it was saturated with $CaCl_2$.

This arrangement gave the following results:

Temperature.	E. M. F.	Temperature.	E. M. F.
1°C	1.91	96°C	1.70
25	1.84	125	1.65
50	1.79	143	1.60
75	1.75	159	1.55

Neglecting the influence of the pressure of the aqueous vapor I have drawn a curve illustrating these results. (See page 24.) Its equation, however, I was not able to make out.

Considering it a straight line, and it does vary greatly from that, it gives the point of dissociation for water at 896°C.

In Watt's[1] dictionary incipient dissociation is said to occur at 960°C.

Here, for the sake of the proximity of the illustrations, and not from any organic connection with the line of thought, I insert an account of some experiments on related subjects.

I. When we mix water and H_2SO_4 a large quantity of heat is evolved. This indicates to us that they have not simply mixed, but that the water molecules and acid molecules have united in some way.

I took 20 c. c. of H_2SO_4 (sp. gr. 1.84) and boiled it until fumes were given off, then allowing it to cool I measured its E. M. F., and added varying quantities of water successively as shown in the table, allowing the mixture to cool each time before measuring.

20 c. c. H_2SO_4 + 0 c. c. H_2O = 1.84 sp. gr. = 1.88 E. M. F.
20 " + .5 " = 1.78 " = 1.96
20.5 " + 1. " = 1.74 " = 1,87
21.5 " + 1. " = 1.71 " = 1.83
22.5 " + 1. " = 1.68 " = 1.82
23.5 " + 1. " = 1.65 " = 1.83
24.5 " + 10. " = 1.46 " = 2.05
34.5 " + 10. " = 1.36 " = 2.02
44.5 " + 10. " = 1.29 " = 1,94

Acidulated water = 1.80 E. M. F.

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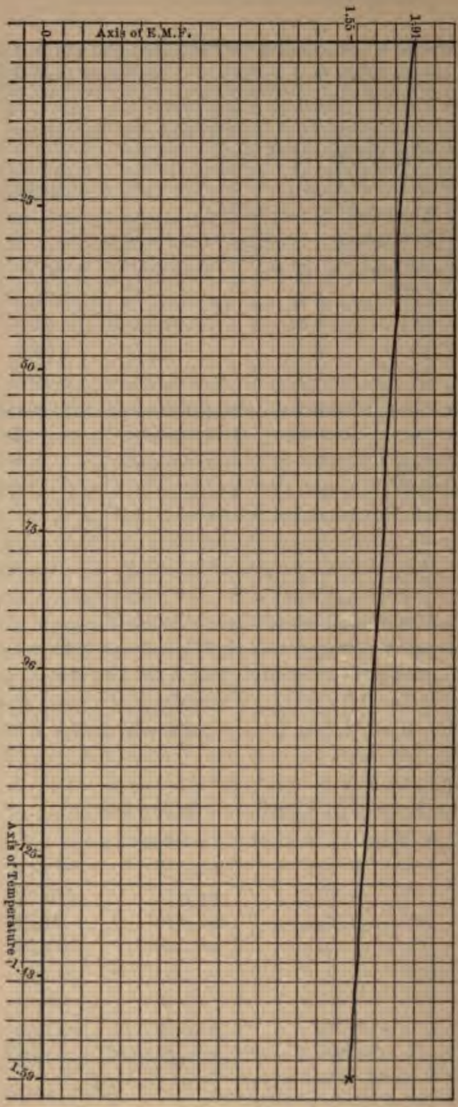
[1] Watt's *Dictionary of Chemistry*, First Supplement—Art. Chemical Action.


Axis of E.M.F.
0
1.55
1.91
25
50
75
96
125
1.43
1.59
Axis of Temperature

I have drawn a curve (page 26, No. 1) illustrating these results. This wavy line seems to indicate that as water is added to H_2SO_4 there are a succession of compounds of water and acid molecules of a varying degree of stability, requiring various degrees of force to break them up.

I thought perhaps a curve showing the resistance of water at various degrees of dilution might show a similar fluctuation, but I find on examination that there is no resemblance whatever between the two curves.

I take some data as given by Maxwell.[1] The numbers are illustrated by a curve on page 26, No. 2.

Sp. gr. of H_2SO_4 at 18.5 C.	Resistance at 22°C. Hg = 1.	Sp. gr. of H_2SO_4 at 18.5 C.	Resistance at 22°C. Hg = 1.
.99	764300	1.26	13132
1.00	465100	1.31	14286
1.05	34530	1.35	15762
1.09	18946	1.39	17720
1.14	14990	1.43	20796
1.20	13133	1.50	25574

It will be seen that the curves are entirely dissimilar.

II. In measuring the E. M. F. of a salt in solution I always aimed to have the solution saturated, but I found that this was not a matter of great importance.

A saturated solution of

NaCl diluted to 20 volumes fell .06 of a volt.
KCl " 20 " " .13 " "
$CaCl_2$ " 20 " rose .06 " "
$CuSO_4$ " 600 " remained constant.

PROPOSITION III.

The electro-motive force of a salt in solution rises as the pressure upon the solution is increased.

This experimental result might have been anticipated from these two considerations.

First. In electrolysis the oxygen and hydrogen (of the water used) pass into the gaseous state, in which change, as we have seen, they absorb energy.

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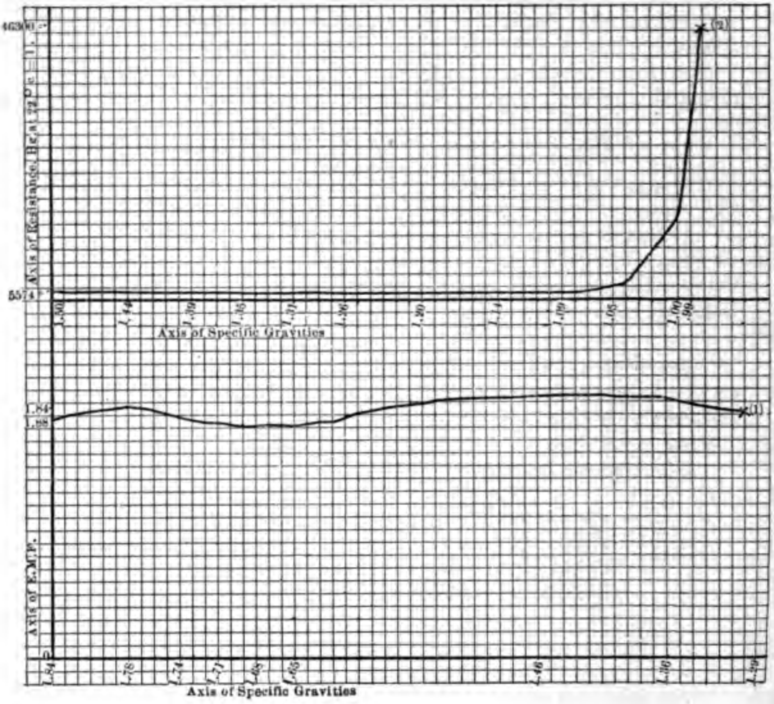
[1] *A Treatise on Electricity and Magnetism*, by J. Clerk Maxwell. Vol. 1. p. 421.


46300
Axis of Resistance, Hg at 22° C = 1.
5574
(2)
1.50
1.44
1.39
1.35
1.31
1.26
1.20
1.14
1.09
1.05
1.00
.99
Axis of Specific Gravities
1.84
1.88
(1)
Axis of E.M.F.
0
1.84
1.78
1.74
1.71
1.68
1.65
1.46
1.36
1.29
Axis of Specific Gravities

Second. The pressure tends to keep them in the liquid condition, hence an augmented force is necessary to overcome this tendency.

In the experiments a glass U-shaped tube was partially filled with acidulated water. Platinum wires with foil upon their ends were then inserted at each end of the tube and hermetically sealed. The enclosed gases thus set free by electrolysis supplied the pressure. This was measured by a small mercurial manometer placed in the tube.

The slightly rarified condition of the enclosed air, owing to the condensation of the vapor generated when the tube was sealed, often, however, lifted the mercury out of the tube, and also, after the pressure had risen to 7 or 8 atmospheres, the indications of the manometer could not be made out.

For these reasons I depended rather upon the time between two readings during which the gases had been generating—hence the hour of the day at which the readings were taken is given.

To increase the quantity of gas generated, a Bunsen battery, of a varying number of cells, was so placed as to send its current through the electrolytic cell in the same direction as the general battery, ("B," page 11.) This additional current could be cut off at pleasure, and was so disconnected just before readings were taken.

These experiments were tried several times, but I transcribe the results of the last two only, as all were similar. "I" is given in pairs.

	I.			II.	
Battery Used.	Time.	E. M. F.	Battery Used.	Time.	E. M. F.
"B"	11.17 a.m.	1.86$_1$	"B"	3.32 p.m.	1.85
"	11.23	1.92$_2$	"	3.40	1.91
			"	3.45	1.91
"	11.25	1.92	"	3.54	1.94
"	11.35	1.96$_3$	"	4.00	1.94
Added 2 Bunsen cells.			"	4.17	1.96
	11.45	1.96	"	4.33	1.97
	12.05	1.96$_4$	"	4.48	1.86
Added another Bunsen cell.			"	5.05	1.86
	12.05	1.96	Added a Bunsen cell.		
	12.25	1.78		5.33	1.69
			Added a Bunsen cell.		
				5.38	1.66

1. About 1 atmosphere by manometer.
2. " 2 atmospheres " "
3. " 4 " " "
4. " 8 " " "

Though another cell was added to "II" it made no appreciable change in the result.

The gradual rise is easily explained on the principle recalled a few minutes ago.

The sudden fall is not so easily explained, but doubtless it was due to the re-combination of the gases thus under a pressure of several atmospheres.

It would seem that it is difficult to start this action, but when once started it takes place freely and just about equals the quantity of gases generated, as the manometer showed no change; and each time a cell was added and more of the gases generated another and lower point of equilibrium was established.

PROPOSITION IV.

The electro-motive force of a single liquid galvanic cell is equal to the excess of the affinity of the acid radical for one plate over its affinity for the other—these latter being measured in terms of electro-motive force.

Suppose that into a cell containing dilute H_2SO_4 we dip two strips of metal—one of zinc, the other of iron. Both begin to dissolve. As it is a synthetic process energy will be evolved, as we have seen, and, in fact, the liquid is heated. We connect the two strips of metal by a wire. The energy that formerly appeared as heat now appears as an electric current flowing from the plate, through the liquid, the other plate and the connecting wire.

But the two plates are sending their currents through the wire in opposite directions. Consequently the *sensible* current will be only the excess of one over the other.

By reference to the table (page 16) it will be seen that:

The electro-motive force of $ZnSO_4 = 2.64$ volts.
" " " " " $FeSO_4 = 2.01$ "
Excess over $ZnSO_4$ over $FeSO_4$ $= \overline{.63}$

To test the trustworthiness of this result I made two small cells, filled with dilute H_2SO_4,—one containing a platinum and a zinc plate, the other a platinum and an iron plate. Placing these in a circuit containing a galvanometer, but with their currents flowing in opposite directions through it, I found, as I expected, that the sensible current was that due to the platinum-zinc element, but its electro-motive force I did not try to determine.

Or again on the same principle:

The electro-motive force of $ZnSO_4$ = 2.64 volts.
" " " " " $CuSO_4$ = 1.62 "
Excess of $ZnSO_4$ over $CuSO_4$ = 1.02

But this is the action of a Daniel cell. Though variously given, the electro-motive force of this element is, according to the best determinations, 1.079 volts.

Though not identical, the numbers are nearly enough so to prove that the principle is correct.

But in this case the copper does not pass into solution, for, as we have already seen, the attraction of SO_4 is greater, for the hydrogen with which it is already united than it is for the copper, yet it is under a tendency so to unite up to the full amount of its affinity for it.

Hence, while no action takes place, a state of stress exists at the copper plate, and this stress exerts the same energy (1.62 volts) in cutting down the sensible current of the battery as though action did take place.

If we substitute platinum for copper we find that the electro-motive force of the cell is still higher, but it is yet under 2.64 volts, showing that even for platinum SO_4 has some affinity. Could a conductor be found which had absolutely no affinity for SO_4, the electro-motive force of such an element, according to these determinations for $ZnSO_4$, would be 2.64 volts.

Thus we see the philosophy of an electro-motive series. And the metals named on page 16 may be arranged in the following order: K, Na, Mg, Zn, Cd, Fe, Cu, Hg, Ag.

When two plates of these metals are placed in H_2SO_4 and connected by a wire, a current flows (in the liquid) from the one higher in the list to the one lower.

By a like study of the chlorides a similar series might be made out for these metals when placed in HCl, and it is easy to see that the two lists might not be entirely identical for all acids.

SUPPLEMENTARY REMARKS ON THE ACTION OF BATTERIES.

A. The electro-motive force of $ZnSO_4$ at 18° C = 2.64 volts.
" " " " " $CuSO_4$ at 18° = 1.62 "
Difference at ordinary temperature = 1.02 "

The electro-motive force of $ZnSO_4$ at 94° C = 2.31 volts.
" " " " " $CuSO_4$ at 94° = 1.36 "
" " " Difference at 94° = .95 "

This would suggest that the electro-motive force of such a cell (Daniel's) would be lower at the boiling point than at the ordinary temperature.

So I made a small CuZn (unamalgamated) cell, placed it in a water bath and measured its electro-motive force at 18° C and 96° C in the same way that I measured the electro-motive force of the polarization current in the electrolytic cell.

I made this determinization twice with the following results:

At ordinary temperature.
First trial, E. M. F. of cell = .971 volt.
Second " " " .987 "
Mean .979

As we have seen the theoretical number is 1.02 volts.

At 94° C.
First trial, E. M. F. of cell = .938 volt.
Second " " " .949 "
Mean .943

The theoretical number is .92 volts.

When we remember that all these data are experimental, consequently liable to error, the results will appear very satisfactory.

B. As we have seen in considering the theory of the method I used in my investigations, the numbers I obtained express the electro-motive force of the electric current generated by the combination of the separated irons.

In the electrolysis of water, either pure or acidulated, these irons are oxygen and hydrogen. Consequently we have something similar in principle to Grove's gas battery, and the E. M. F. of water, as I determined it, should be about equal to that of the gas battery.

For distilled water the E. M. F. is about 2.13 volts.
" acidulated " " " " 1.80 " (page 23).

Sprague (page 224) gives the E. M. F. of Grove's gas battery at 1.50 volts, and quotes De la Rive, who states it to be 2.518 volts. I have not been able to find any statement on the subject in other authorities, but think it quite probable that Sprague is more nearly orrect, as De la Rive wrote quite a while ago and was unacquainted th the exact methods of measurement used by physicists at present.

From what has been said it would be expected that the E. M. F. of the gas battery would vary anywhere between 1.80 and 2.13 volts, depending on the degree of acidulation of the water.

Two considerations may throw some light on the fact that the number given by Sprague is .3 of a volt lower than this minimum.

First. From what we have seen it would be expected that a slight pressure upon the gas would increase the E. M. F. of the battery, but beyond a certain point the E. M. F. would rapidly fall.

Second. In the study of the action of secondary batteries it has been found that the E. M. F. of the current they give is considerably less than that of the charging battery.

Something, possibly a molecular friction analogous to mechanical friction, seems to resist both the charging and discharging of the cell.

Now, as we have seen from the description of the method I used, the direct current from the battery was broken, and I measured the E. M. F. of the polarization current the next instant—catching it at its maximum. It is well-known that this current runs down rapidly.

These two considerations (and the second is the more important) probably explain the number stated by Sprague, but I recall no conditions that, upon this theory, would allow the electro-motive force to rise to the figure given by De la Rive.